

# The Classical Review

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## PAPYRUS FRAGMENTS OF HYPERIDES AND DEMOSTHENES.

THE publication, in the July number of the *Classical Review*, of the fragments of Hyperides in the possession of Mr. Tancock has already had a good result. Mr. Tancock at once took the trouble to apply to the gentleman from whom he obtained the dummy roll in which those fragments were found, and received from him a similar roll which had been bought by that gentleman in Egypt at the same time as the first. Mr. Tancock was kind enough to send this roll to the British Museum for examination, and the result is the discovery of a few more scraps of the large Hyperides MS. (Brit. Mus. Pap. cviii.), together with two minute fragments of a MS. of the *Olynthiacs*. Unfortunately there are no more rolls forthcoming from the same source. By permission of Mr. Tancock photographs of the fragments have been taken, to be preserved with the rest of the MS. in the British Museum, and the texts of the most recent discoveries are here printed.

One of the newly discovered fragments can be attached to frag. *b*, published in July (p. 288). Lines 10-16 now read as follows: ἐπὶ τὴν δι οἰκῆσι ν αὐτοῦ ἄπασαν [ταμ]ίαν ἔχει | ροτόνησ[εν ἐπ]ολαμ | βάνων [χ]άριν α[ὐ] τῶι | π[α]ρ' ἡμῶν ὀφ(ε)ίλε | σ[θ]αι ὅσπερ δίκαιον ἦν, κ.τ.λ. The supplements in ll. 10, 11 are due to Prof. Blass, who also observes that it is Lycurgus who is spoken of, not Demosthenes. ὑπολαμβάνων in l. 14 is corrected in the MS. from ὑπολαμβάνειν.

(c) The next fragment comes from the top of a column, and contains the following letters:—

NO. LVI. VOL. VI.

ΠΕΘΑΙ ΔΣΦ  
ΤΟΝΔΕΙΝΑΙΤ  
... ΤΕΧΡΗΛΑΤ  
... ΡΑΦΥΛΑΤ  
..... ΦΕΡΕ

This fragment may be readily identified as belonging to the top of col. ix. (Blass, ed. 2) of the speech against Demosthenes. The appearance of the infinitive καταλείπεισθαι instead of Blass' conjectural καταλείπομένην ὄρω] necessitates some change in the supplements in the previous column, for which the following may be suggested:—

col. viii. l. 20. φά[σ]κων οὐτε  
τοὺς παρ[ὰ] Φιλοξέ-  
νον τουτο]υσι καλῶς  
ἀξιῶν τὸν Ἄρπαλον  
ἐκδοῦναι τ]ὴν πόλιν,  
25. ἅμα τε νῦν] αἰτίαν οὐ  
μικρὰν τ]ῶι δῆμῳ  
δι' ἐκεῖνο]ν παρ' Ἀ-  
λεξάνδρῳ]ν καταλεί-

col. ix. πεσθαι ἀσφ[αλέστα-  
τον δ' εἶναι τ]ῇ πόλει  
τά τε χρήματ[α] καὶ τὸν  
ἄνδρα φυλάτ[τειν]

5. καὶ ἀναφέρει[ν τὰ χρή-  
ματα κ.τ.λ.]

The two remaining fragments cannot be identified, and it is uncertain whether they belong to the speech against Demosthenes or to that for Lycophron.

(d) Measures 1¼ in. by ¾ in.

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ΝΟΥΦΟ  
ΛΕΙCΛ  
ΕΓΩΔΕ  
ΕΙΤΙΕΙΝ

5 . . Λ . Ε .

(e) Beginning of lines from bottom of a column. Measures (with margins)  $2\frac{1}{2}$  in. by  $\frac{3}{4}$  in.

ΕΙCΔΙ  
ΛΕΝ  
ΤΟΥ  
ΝΟCΟ  
ΗΛΕΡΔ

The two fragments of Demosthenes are from the *Second Olynthiac*. They are written in a large uncial hand, straighter and less flowing than the Hyperides, but neat and elegant in appearance.

(1) From § 10, beginning ὁ ἄνδρες Ἀθηναῖοι. Measures  $1\frac{3}{4}$  in. by  $\frac{1}{2}$  in.

Ω  
ΝΑΙΟ  
ΤΑΚΑ  
ΤΑΚΑ

5 ΝΔΥ  
ΑΙΑΝ  
ΛΛΑΤΑ

No variety of reading is contained in this fragment.

(2) From § 15, beginning καὶ προήρηται πρᾶττων. Contains beginnings of lines. Measures  $1\frac{1}{4}$  in. square.

ΚΑΙ  
ΠΡΑΤ  
ΔΥΝΕΥΩ  
ΒΗΠΑΦ  
5 ΤΟΥΔ . ΑΠ  
/ΤΑΥΤΑΑΛΛΗΔ  
ΠΟΤΕΑΛΛΟC.  
ΔΟΝΩΝΒΑC

The only noticeable point here is the confirmation of the vulgate reading ἄλλος Μακεδόνων βασιλεὺς as against the reading ἄλλος Μακεδόνων adopted by Blass from the scholia.

F. G. KENYON.

#### CONJECTURES ON THE CONSOL. AD LIV. AND ELEG. I. AD MAECEN.

I SEND a few conjectures on the *Consolatio ad Liviam*, and the first *Elegia ad Maecenatem*.

##### 1.

CONSOL. 103 [ed. Baehrens].

Te queritur casusque malos irrisaque †tales.

*Invisaque vita est* Baehrens, *irrisaque vota* Heinsius, *atque irrita vota* Bentley, *incisaeque fila* Withof, *atque irrita tura* Haupt. I propose: *irrisaque tela*, which is much nearer the MSS. than any of these conjectures. 'Scorned weapons,' i.e. the weapons of accident, caused the death of Drusus.

##### 2.

235, 6.

Iste meus periit, periit arma inter et enses  
Et dux pro patria: funera causa †latet.

*Probat* Baehrens, which is far from the MSS., does not account for *latet*, and does not offer a good sense. I propose *levat* or *level*. 'The cause he died for lightens his death': or 'let the cause he died for lighten his death.'

##### 3.

271, 2.

At tibi ius veniae superest, Germania,  
nullum.

Postmodo tu poenas barbaram morte dabis.

*Barbara terra* Baehrens, a violent change. I propose:

Postmodo tu poenas, barbara, *Marte* dabis.

##### 4.

349, 350.

Imposuit te alte Fortuna locumque tueri  
Iussit honoratum: Livia, perfer onus.

Baehrens rightly condemns *Imposuit* and reads *En posuit*. Better, I think, *I! posuit* etc.

##### 5.

375.

Regna deae inmitis parce irritare querendo.

The 'synaloepha ingratissima' may perhaps be removed by reading *mitis*: on the euphemistic principle.

6.

ELEG. IN MAECEN. i. l. 17 (31).

Maius erat potuisse tamen nec velle triumphos:

Maior res magnis abstinuisse fuit.

*Maius erat* has no point: it seems to have come from *maior res* in the pentameter, which *has* point: 'To abstain from great things was a greater thing.' This point is marred by *Maius erat* in the hexameter. Read:

*Maluerat* potuisse tamen nec velle triumphos.

The use of the pluperfect by this writer is quite Propertian. There is no objection to be taken on the ground of *nec velle* following. Even *noti velle* is Ciceronian. *Malueras* is of course equally possible.

7.

31, 2 (39, 40).

Quid faceret? defunctus erat, comes impiger idem

Miles et Augusti fortiter usque sequi.

I fail to see what *defunctus* means here. Now the charge against Maecenas was that he was effeminate in his dress: v. 21 *discinctus...quod carpitur unum*. And here also we should, I think, read *discinctus*. 'What should he have done? he was loose in his dress it is true, but for all that (*idem*),' says the apologist, 'he was a trusty companion and proved soldier.' *discinctus* is almost the same word as *defunctus* in cursives, and in v. 21 some MSS. give *disiunctus* and *discunctus* for it.

A. PALMER.

#### THE PRONOMINAL FORMS *QUIUS*, *QUOIEI*, AND THE PREPOSITION *QUOM*.

##### I. The Forms *quius* and *quoiei*.

Few parts of Indo-European grammar are harder to arrange systematically than the chapter on Pronouns. We find here a perfect wilderness of forms. Different stems express the same pronoun; all sorts of new suffixes and every variety of case-formation occur; intermixture of endings and bold analogies are frequent, while the composition of two distinct pronouns seems not to be unknown. The present paper is an attempt to throw light on two pronominal forms which have already been many times examined, but for which no satisfactory explanation has yet been found.

If we turn to the standard work on Latin Morphology, Stolz's *Latin Grammar*, we find very unsatisfactory statements as to the forms in question. In § 46 he says that *quius* is for *\*queios*; in § 90 we read '*cuius* for *\*quei-i-os*, *quius*,' and half a page lower down he explains *quius* as a nominative *quoi* to which has been added the genitive ending *-os*, *-us*. According to this latter statement, then, we have in *quius* a genitive formed, not from the stem, nor from an already existing genitive, but from a nominative case. Surely Stolz does not believe that himself.

Johannes Schmidt (*Kuhn's Zeitschrift* 25, p. 94) separated *quius* and *cuius* entirely, not considering it possible to derive *cuius*

from *quius*, inasmuch as *quo-* does not change to *cu-* in *quod*, *quot*, *quotus*, and some other forms of the relative pronoun. As the antetype of *cuius* he put a form *\*queius*, which may be the origin of Stolz's *queios*. The change of *que-* to *co-* seems guaranteed in a few cases, as *coquo* < *\*quequo*, but there is not and cannot be any further development of *co-* > *cu-* in accented syllables. The way, therefore, to *cuius* is not made any easier, rather less so, by the assumption of a form *\*queios*. Indeed there is no sufficient reason for separating *quius* and *cuius*. The transition of *quo* > *cu* in the Ciceronian period has been fully established by Bersu (*Die Gutturale n. ihre Verbindung mit V im Lateinischen*, 1885), and is not vitiated by such forms as *quot* and *quotus*, which retain their *o* by reason of the near relationship of the demonstratives *tot*, *totus*. Hence we may dismiss once for all such imaginary forms as *\*queios*, *\*queiei* and confine our attention to *quius* and *quoiei*. These forms are derived by Schmidt from the stem *quio-* = *πιο-*, nom. *πῖος*. The genitive would be *\*quoi-i* > *quoi*, to which another ending *-os*, *-us* was added since the form *quoi* had become identical with the nominative of *quo-*. The form *quoiei* is the regular locative of the same *quio-* and offers no difficulty; see Bersu p. 136.

The existence of a stem *quio-* = *πιο-* has  
G G 2

been again quite lately assumed by Buck in his work on Oscan Vocalism. Buck sets the possessive adjective form *quouis* (*cuius*) =  $\pi\acute{o}\iota\omicron\varsigma$  and derives them both from a locative form plus *-jo*; i.e. *quoi-jo* =  $\pi\acute{o}\iota\omicron\iota\omicron$  (see Buck, *Vocalismus der Oskischen Sprache*, 1892, p. 151). Against such a form for the Greek nothing can be said. It must undoubtedly be correct; but the Latin form is more doubtful. While it is clear that the Indo-European *o* stems had the capacity to form locatives both in *-oi* and *-ei*, there are reasons that lead us to doubt whether the Italic knew any form of the locative save that in *-ei*. These reasons come especially from the Oscan and Umbrian where locatives in *-ei* appear clearly distinguished from datives in *-oi*, *-ui*, and where no trace of locatives in *-oi* appears. Thus the Oscan gives us pronominal forms *atrei*, *piei*, *eisei*, *eizeic*, *exeic*, and similar nominal forms, as, *múinikei terei* = *in communi territorio*, while the Umbrian has such forms as *uze*, *onse*, etc. (Buck p. 155 and 159). For this reason we may not assume too readily a locative *\*quoi*. But even if we allow such a form, other more serious difficulties remain. The transition of a genitive *quouis* to a possessive adjective is much easier to understand than the opposite, which Buck has to assume. True, we have adjective forms used as genitives in *nostrum*, Gothic *meina*, etc., but in these cases the matter is in a very different shape. As Buck himself says (p. 151): 'Hier werden allerdings die Possessivformen mit einer Genitivendung versehen, aber erstens ist das kaum der ursprüngliche Zustand, und zweitens war der Nom. Masc. *quouis* einem Genitiv wie *istius* so ähnlich wie nur möglich.' Dr. Buck plainly sees the weakness of his position and makes a very insufficient defence of it. His conception of the process by which possessives come to be used as genitives would seem to be quite peculiar. It is as if a committee of grammarians had decided to make for the stem *quo-* a genitive out of some form of the adjective *quouis*, and in looking over the field selected *quouis* because in shape and sound it resembled other already existing genitives, as *illius*, *istius*, etc. Why did not that same committee select *mius* (if we may take Charisius's word for this form) instead of *mei*? Even *meus* would be nearer *illius* and *istius* than *mei*. No doubt Dr. Buck's ideas on the subject are clearer and sounder than his language would seem to imply and, if he will pardon me, I will state his position as it seems to me he

ought to have done. It is a well-known fact that a very close relation exists in all the Indo-European languages between the genitive of the personal and the possessive pronouns. We may often assume that the possessives were formed from genitives, or from some forms having the function of a genitive, by the addition of various suffixes (cf. Brugmann, *Grundriss* II. § 450). In this way it is conceivable that a locative *quoi*, doing the work of a genitive, should form an adjective by the addition of the suffix *-jo*, giving a form *\*quouis*. This form having the same meaning as the genitive from which it was derived gradually supplanted that older genitive and left but the one form *\*quouis* for both adj. and gen., the same state of things as we have, for instance, in modern German *unser*. All this seems plausible enough, but an important question arises here. Is it at all likely that the meaning of this *\*quouis* would have been that of a simple possessive adjective? The personal pronouns regularly have possessive adjectives by their side, but the same rule does not hold good either for relatives or interrogatives. The very identity of the assumed *\*quouis* with  $\pi\acute{o}\iota\omicron\varsigma$  shows that the meaning was a general one, not that of a possessive, but rather of a descriptive adjective, such as *qualis*, for example. Nor is it conceivable that this general meaning should have been gradually narrowed until perfect identity with the genitive of the pronoun was established, in view of the fact that not the slightest trace of its original broader and adjectival signification remains. On the other hand this very narrowness of the signification of the adjective *cuius* speaks in the strongest terms for its development from the genitive. It is easy to see how this process was completed. In such sentences as *scibis quouis sis*, Ter. *Heaut. Tim.* 996, we may even yet feel uncertain whether we have a genitive or a nominative. A strengthening of the nominative feeling would cause a fem. and neut. to be formed when necessary, and give us such expressions as *quoia vox*, *quoia res*, *quoum nomen*, etc. The constant transfer of all nominatives to accusatives required by Latin syntax would of course create accusative forms. Into the other cases it spread only partially, nothing occurring but the abl. *quoia* and one instance of the nom. pl. *quouae*. In view, then, of this limitation both as to form and meaning, it seems perfectly clear that the adjective *cuius* is derived from the genitive, and not *vice versa*. Dr. Buck's comparison



of *nostrum* and *meina* is alien to the point, for in these cases the syntactical development was of an altogether different nature (see Brugmann, *Grundriss* II. § 452). Nor is it probable that such forms as *illius*, *istius*, etc., could have had any influence on the development assumed by Dr. Buck; in fact these forms can hardly have been in existence at the time when alone this supposed transfer of *\*quoios* from a descriptive to a possessive, and so genitive, function must have taken place.

A new and rather striking explanation of these forms is that proposed by Brugmann in the second volume of his *Grundriss*, p. 780: "Bereits in urital. Zeit wurde eine erstarrte Casusbildung des Stammes *\*qo-*, etwa *\*kṛō* oder *\*kyōi*, als Interrogativ—bezieh. Relativ-partikel für das flectierte Pronomen so gebraucht, dass die Casusbeziehung durch ein beigesetztes flectiertes Demonstrativpronomen dargestellt wurde, vgl. lit. dial. *tās cēcorius*, *kūr iszválnino jō dūkeri*, 'der Fürst, wo er dessen Tochter befreit hatte = dessen Tochter er befr. h.' So lat. *quoiei* = *qō + eei*, wozu später *quoius* als charakterisierter Genetiv kam, osk. *poizad* = *pō + eizād* (*eizo-* 'is'), pūllad = *pō + ollād* (vgl. lat. *olle*), etc." This explanation may be correct so far as the Oscan and Umbrian forms are concerned. Such forms as pūllad, *poizad*, *pora* are evidently made by combining relative and demonstrative forms, and Brugmann's theory shows how such a combination may have been brought about syntactically. For these Oscan and Umbrian forms we can start from an instrumental *\*qō*, Umbr., Osc. *pō*, = Gr *πω* (*οἷρω*), but this will not do for *quoiei*. The first syllable here must surely have been short or it could not have become *cu-*. Of course we might start with a loc. form *quōi* for the Latin, but then the identity with the Oscan and Umbrian forms is destroyed and the syntactical development falls into the Latin instead of the Italic period. By this the probability of such a development is decidedly lessened in view of the fact that the Latin, from the earliest times, freely used adverbial forms instead of cases of the relative, without ever attempting to add demonstratives to those adverbs in the manner assumed by Brugmann. A few such examples will suffice: *praedonibus unde emerat*, Ter. *Eun.* 1, 2, 35; *hominem, quo illae perveniunt divitiae*, Pompon. ap. Non. 508, 6; *neque quisquam fuit, ubi*, etc., Cic. *Quint.* 9, 34. Another weak point in Prof. Brugmann's theory is the assumption that *quoius* is a new genitive, formed, as a side

piece to the dat. *quoiei*, on the analogy of *illius*, *istius*, etc. Now, if a language allowed such an idiom as, 'der Mann, wo ich dem was geben will,' it would also have used the genitive of the demonstrative in similar manner and said, for example, 'der Mann, wo ich dessen Haus gekauft habe.' And when these two words began to be joined into one, giving us such a form as *quoiei*, we should expect a genitive formed by similar union of relative and demonstrative forms, and not an entirely new and analogical formation. In fact, Brugmann's theory brings us, in its logical outcome, to the theory proposed by Meunier (*Mémoires de la Société de linguistique*, 1, 14), who assumed both for *quoius* and *quoiei* a composition of the relative with the demonstrative pronoun *is*, analysing the forms thus, *quōi-ius*, *quōi-ei*. But the insuperable difficulty here is, that for such a form as *ius*, gen. of *is*, we have not the shadow of authority.

It seems to the writer of this paper that a very simple and satisfactory explanation of these forms lies close at hand. The form *quoius* must not be separated from other genitives in *ius*, as *alius*, *illius*, *istius*, *eius*, etc. These latter forms are clearly double genitives, that is to say, to a locative *isti*, having the function of genitive, as well as locative or dative, was added the nominal genitive ending *-os*, *-us*. Now let us assume that side by side with the locatives *isti*, *illi*, *alii*, *ei* (<*\*eiei*, stem *eio-*), there existed a form *quōi*, in which the diphthong *oi*, being accented, had not yet suffered any change. The form *quōi* I prefer to consider a genitive, but those who believe in the existence of Italic pronominal locatives in *-oi* may call it a locative. To all these forms there was added at the same time and under the same influence the characteristic genitive ending *-os*, *-us*, so that we have *quoius*, *illius*, *istius*, *eius*, *alius* <*\*alius*. In these latter words the old locatives still remained as datives, but for *quōi* the locative *\*quēi*, if we may assume that such a form once existed, had now fallen out of vital connexion with the genitive, and so a new dative *quōi*, or *quōiei* (>*quōi* > *cui*) was made by analogy to the relation existing between *alius*, *alii*, etc. It may be that the form *eiei* (*eiei*) is in like manner a new dative to *eius*, but its natural development would bring it back to the original form (*eiei* > *eī*) just as *quōiei* > *quōi* > *cui*.

In the explanation just given there are one or two points that call for a few

words of comment. The assumption of *quōi* and *istī* existing side by side is in perfect accord with what we know of the early development of the Latin diphthongs. The diphthong *-ei* had become a monosyllable before the beginning of our records, while the change of accented *oi* to *ū* takes place under our very eyes, the forms *oi* and *oe* being abundant in early inscriptions and the final change to *ū* not being completed before the middle of the seventh century u.c. (Stolz § 34). There still remains, however, a form *quōi*, used as a genitive, that must be provided for, although it would seem that this form is nowhere above suspicion. In the African Inscription, *cui non misertus ego*, where Bücheler (*Lat. Declin.* p. 39) finds a genitive, we have really a dative (cf. *Neue IL*<sup>3</sup> p. 452). Probably *quōi* in Plaut. *Trin.* 1126 is the surest example of such a genitive (cf. Brix *ad loc.*): in *Mil. Glor.* 1081, where B has *cui*, the other MSS. have *cuius*, and in *Most.* 962, where BCD have *quōi*, A has *cuius*. The compound *cuiusmodi* occurs quite frequently in Gellius, but the form *cuiusmodi* (*quōiquōimodi*) does not seem to be recognized at all by Georges in his *Lexicon der Wortformen*. But even if we allow *quōi* as a genitive the explanation is not difficult. Of course it is not the original *\*quōi* which was assumed as the basis for *quōius*; it is rather the new dative *quōi* < *quōi*. This came about in this way. The old locatives *illi*, *isti*, etc., were used both as genitives and datives, and even after the formation of the new genitives *istius*, *illius*, the old forms survived in certain phrases, as *isti modi*, *illi modi*. Now, inasmuch as *quōi* < *quōi* was felt to be identical in case function with *illi* and *isti*, it came to be used as a genitive just as those forms were so used. What form the dative of *quo* had before the new form *quōi* arose is a matter of speculation. The analogy of *illi*, *isti*, etc., would point to a locative form *\*quēi*. Or, if we may regard it as possible that in such datives as *uno*, *alio*, *isto*, etc., old formations survive, it would be easy to see the old dative *quo* in some constructions usually explained by the help of the adverb, as *quo illae nubent*, Plaut. *Aul.* 3, 5, 13; *quo dedisti nuptum*, Stich. 142; *quo iam diu sum iudicatus*, *Menaechni* 96.

## II. The Preposition *quom*.

It is a well-known fact that the early inscriptions have the form *quom* for the

preposition as well as for the conjunction (cf. Brambach, *Neugestaltung* p. 223). That this is not the proper etymological form is perfectly clear. Oscan *com*, kum, Umbrian *com*, kum, ku (asaku), prove the Italic form to have been *kom*, and this is further supported by the Old Irish *com*- and the Greek *κοινός* < *\*kom-ios*. The form *quom* has always been explained as an example of the writing of *qu* for *c*, such as occurs for instance, in *oquoltod C.I.L.* I. 196; *hoiusque* I. 603; *quolundarum* in a Faliscan Inscription, Zvet. 70 b. This would be the natural explanation if we had simply one or two examples of *quom* side by side with others with the proper spelling *com*. As it is, however, the case is more than reversed. The early inscriptions know only the form *quom*; *com* does not occur at all. Bersu in his work on the Latin Gutturals (p. 42) has collected the examples of *quom* in the inscriptions, yet he seems to hesitate when the time comes to draw the only conclusion possible from his material. True, he speaks of *cum* being developed out of *quom* (p. 53), but he seems to approve Corssen's moderation in not assuming labialization of the initial consonant for the preposition, 'und er hätte hier doch noch am ersten einen entfernten Schein von Berechtigung gehabt, da es sich aus älterem *quom* entwickelt hat' (p. 51). Why 'einen entfernten Schein'? If *cum* is developed out of *quom* alike for preposition and conjunction, then they must have been pronounced alike, and Corssen would have had, not 'einen entfernten Schein von Berechtigung,' but a perfect right to assume labialization of the initial consonant. Again (p. 45) Bersu says: 'In allen diesen Fällen (*quolundarum*, *oquoltod*, *quom* praep.) scheint der labialisirte Guttural ebenso wie der labiallose in *qui* nur einer orthographischen Verwechslung seine Entstehung zu verdanken, der Sprache aber ursprünglich fremd zu sein.' This 'ursprünglich' leaves us again in doubt just how far Bersu meant to go. Since Bersu's work appeared the point has not, I believe, been touched upon by any one. Stolz (*Gram.* p. 288), though he had Bersu's material before him, still pronounces *quom* a 'Schreibfehler.'

Let us see what the facts are in the case. The form of the preposition *quom* occurs in an inscription falling between the years 532 and 602 u.c.; again in one about the year 600; four examples in 631—2; one in 643 and one in 664. During this century and a half not a single example of the original *com* is found. From about the year 700 u.c.

begins the transformation of *quo* > *cu* seen so plainly in the conjunction *quom* > *cum*, and hand in hand with this development of the conjunction goes the change of the preposition *quom* to *cum*. The first example of the conjunction *cum* occurs in 709 u.c., of the preposition in 705. For a while the two forms *quom* and *cum* exist side by side, then *quom* disappears altogether or is only retained through an archaizing tendency. From all this it appears very plain that we have in the preposition a real change of sound from *com* to *quom*, caused by identifying the preposition with the conjunction. Analogies for this identification were at hand in several pairs, as *ergo*, *sed*, *ad* (*at*). It was a kind of folks-etymology by which the two words were brought more closely together. That such an occurrence was quite possible we may easily convince ourselves by observing the effect of folks-etymology in living languages. Even in the Latin we may find a number of other examples that illustrate the principle (see in general Keller, *Lat. Volksetymologie und Verwandtes*, 1891). Thus *calx*, *chalk* = *χάλιξ* should have the form \**chalx*, but joins itself to an already existing *calx*, *heel* (p. 67); *sarcophagus* becomes in vulgar idiom *sacrophagus* through the influence of *sacer* (p. 128); *sepulchrum* takes its *h* from *pulcher* (p. 128); *cloaca* gets the vulgar form *coacula* through supposed connexion with *coagula* (p. 131, Schuchardt, *Vulgärlatein* III. 312); *atqui* and *alioqui* are treated to a final *n* because the last syllable is identified with *quin* (p. 144). A striking instance of the same change of *c* > *qu* may be seen in the attempt made by scholars in the first century of our era to change *cottidie* to *quotidie*, not only in orthography but also in pronunciation, because of a supposed etymology *quot diebus* or *quoto die*; cf. Quintil. 1, 7, 6.; Vel. Long. 79, 16.

With regard to *cum* it must be borne in mind that the change discussed only affected the independent preposition. When appended to pronouns the form *-com* changed to *-cum* at the same time that other unaccented syllables in *o* were similarly affected (cf. Brugmann, *Grundriss* I. § 81). For that reason the earliest example found, *secum* from 631—2 u.c., shows already the form *-cum*. In composition we have *com-*, not because, as Bersu says (p. 42), 'inlantes *o* nicht betrübt war,' which is not true, but because the unaccented prefixes follow the analogy of the accented. Forms like *cōsfugit*, *cōnficit*, *cōnfluit* are perfectly regular, and from these unaccented *o* keeps its place in *confugio*, *confacio*, *conflüere*, etc.

The forms of the independent preposition with *o* cited by Georges and Neue are all vulgar and late. They are not a survival of original *com* but a change from *cum* to *com*, as we see in Italian and Spanish *com* (cf. Schuchardt, II. 166). These inscriptions with *com*, *con*, *co* are provincial, being largely from Dalmatia, Pannonia, and the regions round about Trieste and Milan, with one or two from the vicinity of Naples.

In conclusion one word as to *quom* in our Manuscripts. If this form of the preposition was the common and established one, as the inscriptions indicate, we should expect to find traces of it in some of the Manuscripts of works whose authors must have used it. And such is indeed the case. Though the evidence for the preposition *quom* is not as great as that for the conjunction, yet it is still considerable. The new edition of Neue attests *quom* (prep.) in 18 passages from Plautus and in six from Cicero, and it can hardly be doubted that a careful search would reveal many others.

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#### THE REMOTE DELIBERATIVE.

MR. SIDGWICK bases his argument for the introduction of that grammatical novelty the Remote Deliberative Optative on 'the fact that all the passages where commentators have regretted the want of *äv* in Attic Greek have one common character; they are all *interrogative* either direct or indirect' (Aesch. *Ag. App.* I.). Should it turn out therefore on examination that many of the

examples cited as remote deliberatives are not interrogative in any sense of the word, Mr. Sidgwick's induction must be pronounced as incomplete as that of Paley, who somewhat hastily ascribed to all a *negative* character. Now it seems to me that, with a single exception, the examples brought forward by Mr. Sidgwick as indirectly interrogative are in no sense interrogative, but are merely

relative clauses with the antecedent omitted. The exception alluded to is οὐκ ἔχω πῶς ἀμφισβητοῖν. Οὐκ ἔχω, if not practically identical with οὐκ οἶδα, or only slightly shaded off from it, is at least analogous, and may therefore be regularly followed by an indirectly interrogative clause. As much cannot be said of any of the other examples cited, without doing violence to their analysis and without disregarding the cogency of parallel expressions. If we analyse these sentences we find they consist of a principal clause ἔστιν or οὐκ ἔστιν, with a subordinate relative clause introduced by ὅς, ὅστις or ὅπως. Thus οὐκ ἔστιν ὅτῳ νείμαμι = οὐ (τις) ἔστιν ὅτῳ κ.τ.λ., οὐκ ἔστιν ὅπως... is equivalent to οὐκ ἔστι μηχανὴ ὅπως (= ὁποῖα). Sometimes the antecedent appears and instead of the equivocal ὅστις we find ὅς which is almost exclusively *relative*, e.g. καὶ τίς ποτ' ἔστιν ὃν γ' ἐγὼ ψίξαμι τι; *who* is there *whom*...?

On the other hand in translating into Latin or English we must deal with them as *simple* sentences, treating ἔστιν ὅπως etc. as mere periphrases for adverbs or pronouns. Thus οὐκ ἔστιν ὅπως = οὐδαμῶς; οὐκ ἔστιν ὅστις = οὐδεὶς. In this way we find ourselves among a large and well-known class of periphrastic expressions, which are all ultimately reducible to relative clauses. Such are εἰδὼν οἷ (ἔστιν οἷ) = some (there are *those who*); ἔστιν ὅπου = somewhere (there is a place where); ἔστιν ὅτε = sometimes (there are times when); ἔστιν ἥ = somehow, οὐδεὶς ὅστις οὐ = everybody; οὐκ ἔστιν ὅπως οὐ = most assuredly; οὐκ ἔσθ' ὅπως = by no means.

Of the foregoing, those with which we are now concerned are chiefly followed by the indicative, future or present. It may seem superfluous to cite examples, yet I may perhaps be pardoned for quoting a few.

οὐ γὰρ ἔσθ' ὅπως πόλιν | κείνην ἐρείψει.  
Soph. *Oed. Col.* 1372.

Never canst thou overthrow that city  
(Jebb).

ταύτην ποτ' οὐκ ἔσθ' ὥς ἐτι ζώσαν γαμεῖς.  
Soph. *Ant.* 750.

οὐκ ἔστιν ὅπως καταπαύσει.  
Eur. *Med.* 171.

οὐ γάρ τις ἔστιν ὃς πάροιθ' ἀῖρήσεται.  
Eur. *Heracleid.* 57.

ἔσθ' ὅπου τὸ δεινὸν εὖ.....  
δεῖ μένειν καθήμενον.  
Aesch. *Eum.* 517—19 (Sidgwick).

Somewhere should fear abide enthroned  
(Sidgwick).

οὐκ ἔσθ' ὅπως οὐ Χαλκιδέας ἀφίστατον.  
Arist. *Eq.* 238.

But though the indicative is more fre-

quent, the potential optative is also used, as might be expected. The difference between οὐκ ἔσθ' ὅπως λέξεις and οὐκ ἔσθ' ὅπως λεξείας ἄν lies in this—that whilst the former denies the *fact* (i.e. the means of its attainment), the latter denies even its *possibility*. Thus they both constitute strong denials. In like manner ἔσθ' ὅπως λέξεις; = will you say? i.e. is there any chance of your saying? ἔσθ' ὅπως λεξείας ἄν; = would you say?

The following are instances of optative with ἄν:—

ἔσθ' ὅτῳ ἄν ἄλλῳ ἰδοίς ἢ ὀφθαλμοῖς;  
Plato *Rep.* 352 E.

οὐκ ἔσθ' ὅτου | ὀργῇν ἔχοις ἄν.  
Soph. *Phil.* 1309.

Thou hast no cause of anger (Jebb).<sup>1</sup>  
οὐκ ἔσθ' ὅτου θίγοι μ' ἄν ἐνδίκωτερον.  
Eur. *El.* 224.

If we place side by side with these examples the passages without ἄν quoted by Mr. Sidgwick, we shall be at a loss to discover any difference—at least perceptible in translation. Οὐκ ἔσθ' ὅπως λέξαμι ἄν would, in the light of the foregoing, be of identical force with that of the similar passage in the *Agamemnon*. The conclusion naturally suggests itself that, in those so-called remote deliberatives, the particle ἄν is omitted, just as it is not unfrequently in Epic and other non-Attic writings.<sup>2</sup>

To say nothing of Homer, instances of this omission are met with in Pindar and Theocritus:—  
Αἴσωνος γὰρ παῖς ἐπιχόριος οὐ ξενίαν ἐκοίμα γαῖαν ἄλλων.

Pind. *Pyth.* iv. 118.  
νανσί δ' οὔτε πεζὸς ἰὼν τάχ' εὐροῖς ἐς Ὑπερ-  
βορέων ἀγῶνα θαυματὰν ὁδόν. (Sic. codd.)

Pind. *Pyth.* x. 30.  
Θεός ἐῖ ἀπήμων κέαρ.

Pind. *Pyth.* x. 21.  
οὐ μιν διώξω· κεινὸς ἐῖην.

Pind. *Ol.* iii. 45.  
οὕτως ἐπὶ ματέρα νεβρὸς ἄλοιο.

Theocr. viii. 89.  
αὐτὰρ ἐγὼ Πτολεμαίων, ἐπιστάμενος καλὰ εἰπεῖν,  
ὑμνήσαι μ'.

Theocr. xvii. 7.

<sup>1</sup> Cf. also the potential indicative  
οὐκ ἔστιν ἥτις τοῦτ' ἄν Ἑλληνὶς γυνή | ἔτλη ποθ'.  
Med. 1339—40.

<sup>2</sup> A well-known feature of Epic Grammar is the omission of ἄν in indefinite clauses, temporal and relative e.g. with ἐπεὶ, ἐπειδὴ, μέχρι οὗ, πρὶν, ὅς, ὅστις, etc., followed by a subjunctive: also in conditional clauses (εἰ σοῦ στερηθῶ). This usage has found its way into Attic drama and is even met with in Thucydides. Might not the poets have dealt similarly with the potential optative for metrical convenience or sententious brevity?



ἐν εἰκοσι πᾶσι μάθ' οἷς νιν.

Theoc. (i)

τάχα δ' ὕστερος οὐδ' ἄλα δ' οἷς ης.

Theoc. xxvii. 61.

καὶ τί φίλος ῥέξαιμι; γάμοι πλήθουσιν ἀνίας.

Theoc. *ib.* 25.

Cf. also Aesch. *Eum.* 265 (Sidgwick).

ἀπὸ δὲ σοῦ

φ' ε' ρ ο ἰ μ α ν βοσκὰν πώματος δυσπότου.<sup>1</sup>

The omission takes place for terseness even in prose when the reader is expected to pick up the *ἄν* from a previous sentence: cf. Plat. *loc. cit.* *ἔσθ' ὅτ' ἄν ἄλλω ἴδοις ἢ ὀφθαλμοῖς*; *Οὐ δὴτα. Τί δὲ ἀκοῦσαις ἄλλω ἢ ὤσιν*;

Mr. Sidgwick admits that the clauses he terms indirectly interrogative are not strictly such, but only by analogy. The analogy however is far to seek. An oblique question is essentially a question repeated in word or thought; it is therefore necessarily dependent on some verb or verbal substantive implying perception of mind or sense or the outward expression of such perception (*verba sentiendi et declarandi*). In these indirect deliberatives no such introductory statement occurs; neither can it be understood.

In the sentence *οὐκ ἔχω ὅ τι εἶπω*, which is quoted as analogous, this connecting link appears: *οὐκ ἔχω* (I am at a loss to know—to find) implies at least something like mental effort, whereas the introductory clause *οὐκ ἔστιν*, in the passages under discussion, denotes exclusively existence or actuality. The *ἔστιν* here is not copulative, as is seen from the analysis and as may be further shown if we endeavour to supply some word akin to a 'verbum sentiendi' or 'declarandi.' For instance *οὐκ ἔσθ' ὅπως λέξαιμι* cannot be rendered 'there is no question how I am to speak,' nor 'there is no thinking how I could speak,' nor 'there is no saying etc.': none of these versions will give the required meaning. It is not the question 'how I am to say,' nor the *discovery of means* 'to say' that is denied; it is simply the future contingency of 'saying.' To have a genuine ring about them our examples should be of the following type *minus ἄν* :—

<sup>1</sup> This can scarcely be regarded as a wish, seeing that the blood-sucking punishment was already declared to be the special prerogative of the Furies and a thing positively to be accomplished. People do not wish for what is already their own.

οὐκ ἔχω πῶς ἄν | στέρξαιμι κακὸν λείσσω

Soph. *Trach.* 992.

οὐκ οἶδ' ὅπως εἴποιμ' ἄν.

Eur. *Hipp.* 981.

But for the inopportune presence of *ἄν* these would be admirable examples of the remote deliberative.

As regards the almost unique prose instance *οὐκ ἔχω πῶς ἀμφισβητοῖν*, most editors would unhesitatingly follow Professor Jebb in restoring *ἄν*, on the principle that the oversight of a copyist is a more plausible explanation of a manifest anomaly than the imputation of inconsistent grammar to Plato.

II. There remain the instances of the simple interrogative without *ἄν* such as *ποῖ τις φύγει*; Of these I will say nothing, as they have, with two exceptions, been rejected by so great an authority as Professor Jebb. This eminent scholar follows in the wake of former critics in altering to the subjunctive or inserting *ἄν*. In the two following lines he pronounces the optative alone to be sound :—

τέαν Ζεῦ δύναισι τίς ἀνδρῶν ὑπερβασία κατὰ  
σχοι; Ant. 665.

ἀλλ' ὑπέρολμον ἀνδρὸς φρόνημα τίς λέγοι;  
Aesch. *Cho.* 595.

Both these may be classed as cases of omission of *ἄν*, on the grounds previously set forth.

When I say that *ἄν* is omitted all I wish to imply is—that in the Greek of Pericles or Demosthenes the vast majority of sentences similar to those where it is missing would have it. I do not wish to assert that the optative alone was not used originally in clauses, affirmative, negative and interrogative, to express a statement put forward as a pure conception. The insertion of *ἄν* may have been an accretion to distinguish positive statement from the expression of a wish.

In conclusion it seems to me most undesirable to incorporate the remote deliberative into our grammars side by side with the optative of wish or mild command. The scholastic adage '*entia non sunt multiplicanda sine necessitate*' is nowhere more applicable than in grammatical criticism. We must generalize from the broad facts of language and not base our theories on a few isolated anomalies.

J. D.



## FARNELL'S GREEK LYRIC POETRY

*Greek Lyric Poetry*, a complete collection of the surviving passages from the Greek song-writers, arranged with prefatory articles, introductory matter, and commentary by GEORGE S. FARNELL, M.A. London: Longmans Green and Co. 1891. Svo. Pp. xvi. + 490. 16s.

It was time there should be some English edition of the Greek lyric poets. This book fills a vacant place, and though unsatisfactory in some respects, has also some distinguished merits. The prefatory essays deal with the history and characteristics of Greek lyric poetry. There is also a careful chapter on dialect, and philological observations are a marked feature of the notes. Mr. Farnell has further done well in giving an account of the conclusions attained by modern German metricians so far as they concern the present subject. This is sympathetically written, with intelligence and lucidity. The fragments of each author are also preceded by brief historical and critical remarks. These chapters together form a good introduction to Greek lyric and are the best part of the book. Mr. Farnell tells us that his 'object in this volume has been to present to readers of Greek a collection in an accessible form of all the fragments of the "Melic" poetry, omitting from the text instances of single words or half lines cited in illustration of some special point in grammar or metre, and also passages which are hopelessly corrupt.' But 'to make the collection complete for purposes of reference, etc., I have added in an Appendix all the passages excluded from the text proper. These latter I have taken from the last edition of Bergk's *Poetae Lyrici*, without commentary or alteration of the text.' This Appendix fills thirty pages and, whatever readers of Greek it be meant to benefit, the addition of it seems a mistake. The scholar will read these passages in Bergk, where he can see what the texts have and in what setting the fragments are found, and for purposes of reference he will use Bergk: while readers to whom Bergk is not a necessity are not likely without a word of help to make much for instance of the *Δείπνον* of Philoxenus.

The commentary is somewhat slight, but well enough in general, though where himself not depending on other critics Mr. Farnell is not always a safe guide.

In Alcman 87 ἀνὴρ δ' ἐν ἀρμένουσιν ἀλιτρός ἦσ' ἐπὶ θάκω κατὰ πέτρας ὀρέων μὲν οὐδὲν δοκέων δέ, it is impossible that ἐν ἀρμένουσιν (Bergk and Hecker for ἐν ἀσμένουσιν) can have either of the two meanings between which Mr. Farnell permits a choice, 'in bonds' or 'among those bound': it must mean 'in comfort,' 'amid good cheer.' The more usual form of phrase would be ἐν ἀρμένουσι πᾶσι (which is rightly restored from the Schol. by Hecker in Pind. *N.* iii. 58) as ἄρμενα πάντα (Hes. *Op.* 407, *Scut.* 84, Theognis 275), ἀγαθὰ πάντα, ἐν πᾶσιν ἀγαθοῖς (both frequent), ἀφθονὰ πάντα (h. Hom. *Ap.* 536, Theocrit. viii. 40), ἐν πᾶσιν ἀφθόνοις (Xen. *Anab.* iv. 5. 29, Lucian iii. 341, 374); but we find also Xen. *Anab.* iii. 3. 25 ἐν ἀφθόνοις βιοτεύειν, Dem. 312. 18 ἐν ἀφθόνοις τραφεῖς.

In Timocreon 1 οἱ δ' ἦσθιον κηύχοντο μὴ ὦραν Θεμιστοκλέος γενέσθαι Mr. Farnell without remark translates 'that the day of Themistocles might be no more, i.e. that his ascendancy might come to an end.' This at any rate is impossible. Even if ὦραν Θεμιστοκλέος could mean 'the day of' μὴ is misplaced. We must have had μὴ γενέσθαι together; and these words could not mean 'come to an end' (μῆκετ' εἶναι), but only 'not come into being.' As it is, the order of the words implies 'that there might be, come into being, be made, be taken, no . . . of Themistocles,' and this is completely solved by Ahrens' ὦραν. ποιῆσθαι τινος ὦραν (e.g. Hdt. ix. 8, Lucian ii. 372, Dionys. *de comp. verb.* 21), λόγον (Hdt. *passim*, Theocrit. iii. 33) have for their passive γίγνεται τινος ὦρα (Tyrtaeus 10. 11 εἰ δ' οὕτως ἀνδρός τοι ἀλωμένου οὐδεμί' ὦρῃ γίγνηται), λόγος (Hdt. i. 19, viii. 102, ix. 80, *A.P.* v. 280 ἡμετέρης δέ φροντίδος οὔτε λόγος γίνεται οὔτ' ἀριθμός). Bergk indeed hesitates between this and χώραν in the same sense, referring to his note on Theognis 152. But there can hardly be any doubt that one of the two is right.

The note on Pindar *fr.* 58. 8 ἐνθα τεκοῖα' εὐδαίμων' ἐπόψατο γένναν, 'a fine example of Pindar's terse descriptive power, a picture of the mother's fond gaze on her "goodly offspring" being called up by a single stroke,' merely misses the sense of ἐπόψατο 'lived to see,' 'came to the sight of,' which is very frequent, and frequent also in just a like connexion, e.g. Hdt. vi. 52, Eur. *Med.* 1012, Xen. *Cyrop.* viii. 7. 7, *de vect.* 6. 1, Herodas v. 70.

On Archilochus 56 τοῖς θεοῖς τίθει <τὰ> πάντα Mr. Farnell says 'For τίθει, Bergk compares Aesch. Pers. 424, ταῦτα πάντα θύομεν θεοῖσι' [the line, as Bergk gives rightly, is 224, 231 Weckl.]: and on 68 Λεωφίλῳ δὲ πάντα κῆται cites θεῶν ἐν γούνασι κῆται, which is no parallel. The two fragments best illustrate each other; in the one we have the active form of the phrase, in the other the passive.

Mr. Farnell has an unfortunate habit of inaccuracy. A papyrus and a parchment are not the same thing, though in speaking (p. 308) of the Egyptian papyrus containing Aleman *fr.* 23 Mr. Farnell twice calls it 'the parchment,' using both words as though they were synonymous. Such errors as 'demonstrated, as Bergk points out, by Terentian *Maur.* 2154' (p. 335), 'Mr. Swinburne makes much of this line in his *Anastasia*' (p. 335, for *Anactoria*—and it is not there but in *On the Cliffs* in *Songs of the Springtides* that Mr. Swinburne makes much of this line), 'Byron in his translation of this song "My wealth's a burly spear and sword"' (p. 385, for Campbell and 'My wealth's a burly spear and brand'), though not important in themselves have their significance, and will not inspire confidence in an editor. But 'The well-known lines of Sophocles will suggest themselves, Τίς δ' οἶδεν εἰ τὸ ζῆν μὲν ἔστι καθαρεύ κ.τ.λ.' (p. 413), implying all it does, is a serious error which it should not have been possible to make.

It is to be hoped that this book may be found profitable, and no doubt there is profit to be obtained from it. At the same time

I cannot help doubting whether the plan of it was well-advised. It appears to be intended for readers to whom Bergk's great edition is indigestible, and of such there is no question a large and growing number to take pleasure in this exquisite literature. But if the book was to be made really serviceable to these, Mr. Farnell should in the first place have given a translation, as Mr. Mackail has lately done for his selection from the Greek Anthology. It may however have been felt that the addition of a translation was inexpedient for the purpose of the school-master. But the book is not adapted to the use of schools. It is at once too full in the text and the disquisitions on metre and dialect, and too meagre in the annotation. For a school-book there can be no better model than the *Anthologie aus den Lyrikern der Griechen* by Dr. Buchholz, now in its third edition. This, which Mr. Farnell too seldom mentions (four times, I think, only altogether), is in two parts, one containing selections from the elegiac poets and iambographers, the other selections from the melic and choric poets. If a selection from these is desired for English schools, nothing could be better than a translation of that volume, adapted to English books of reference. But the present edition, with all its merits, I fear is not successfully designed to satisfy the wants of any class of readers. If Mr. Farnell wished to be helpful to older amateurs, he should certainly have added the translation. Perhaps he may yet find the opportunity for doing so.

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#### HERBST'S NOTES ON THUC. I.—IV.

*Zu Thukydides. Erklärungen und Wiederherstellungen* von L. HERBST. Buch i.—iv. Erste Reihe. Leipzig, Teubner. 1892. xii. 124. 2 Mk. 80 pf.

THE veteran critic Herbst celebrated his eightieth birthday on the last day of June 1891. The inevitable *commentationes* appeared, both *seorsum* and *collectae*. Taken together they fill just twenty-four quarto pages. A few months—eight or ten—pass; and, behold! forth from the press comes part I. of the Professor's return gift, occupying one hundred and twenty-four octavo pages, not to mention twelve pages of preface!

But those twelve pages deserve a mention. For they contain, not explicitly indeed, but by implication, the solution to a problem propounded by Fr. Müller in *Bursian's Jahresbericht* for 1889. 'Why is it,' asked Müller, 'that from Herbst we have no edition of Thucydides?' And here Herbst writes: 'Die Herausgeber eines ganzen Werkes pflegen an tausend Stellen in der übelsten Nothlage zu sein. Ueberall fühlen sie den Beruf, sich auszusprechen, auch da schon ein fertiges Urtheil zu haben, wo es bei ihnen erst zum blossen Meinen gekommen ist.' Herbst's method is not Classen's. We should perhaps have preferred to find more of Cobet's literary sense in Cobet's

adversary, even at the risk of losing some of the rules and statistics to which he has helped us so liberally. But, after all, let the method be what it may, a critic who first wrote on Thucydides somewhere near the date of the completion of Poppo's first edition, and who, more than half a century later, is still pursuing serenely the method on which he started, demands from us younger men profound respect and patient consideration.

Some sixty passages are dealt with, and in all cases great weight is, of course, attached to manuscript authority. Acute remarks 'by the way' are to be found here on almost every page. In some few of his proposals Herbst has been anticipated. A very attractive emendation is *ναὺς ἐπετάχθη σ' ποιέσθαι* for *ναὺς ἐπετάχθησαν ποιέσθαι* in ii. 7, but much the same idea occurred many years ago to Donaldson. And two or three of the difficulties solved have been already solved in the same manner by F. Müller. But this is nothing. I note that Steup in his edition of Book iii. gives an opinion adverse to all Herbst's new proposals in the interpretation of that book, but he gives no reasons for his sentence.

It will perhaps be best to confine our attention here to one book—and I take the second. In c. 15 Thucydides gives two proofs that the most ancient city was built on the Acropolis and south of it. (1) *τὰ γὰρ ἱερά ἐν αὐτῇ τῇ ἀκροπόλει καὶ ἄλλων θεῶν ἐστὶ κ.τ.λ.* As the temples might have been modern, the text contains no proof whatever of the thesis. It has been usual to assume a lacuna after *ἀκροπόλει* with Classen: but Herbst reads *τὰ γὰρ <ἀρχαῖα> ἱερά.* (2) καὶ τῇ κρίνῃ τῇ νῦν μὲν τῶν τυράννων οὕτω σκευασάντων Ἐννεακρόνῳ . . . τὸ δὲ πάσαι φανεράν τῶν πηγῶν οὐσῶν Καλλιρρόῃ ὀνομασμένην, ἐκείνῃ (Bekker and subsequent edd. *ἐκείνοί*) *τε ἐγγὺς οὖσῃ τὰ πλείστον ἄξια ἐχρῶντο, καὶ νῦν ἐτι ἀπὸ τοῦ ἀρχαίου...νομίζεται τῷ ὕδατι χρῆσθαι.* Herbst insists that the antithesis here lies between *ἐκείνῃ*, the spring when it was close to the inhabitants and therefore convenient for use, and *τῷ ὕδατι*, which is inserted just because Thuc. is now contemplating the water as remote from the inhabitants and requiring to be brought. But where is the proof required? 'Formerly the spring was open, and men used that spring on important occasions because it was near, and even now it is the custom to use it' etc. The text cannot be sound. 'The spring was once in *general* use because it was then near. Even now its water is used on special occasions (though it has to be brought from

some distance).' That is what we require; and Herbst's remarks are to a great extent irrelevant. But anyhow there is a different nuance of meaning here in *τῇ κρίνῃ χρῆσθαι* and *τῷ ὕδατι χρῆσθαι* which explains the insertion of *τῷ ὕδατι*.

In c. 16 *τῇ τε οὖν ἐπὶ πολὺν...αὐτονόμῳ οἰκῆσει [μετείχον] οἱ Ἀθηναῖοι, καὶ ἐπειδὴ ἐνωκίσθησαν κ.τ.λ.*, he makes *τε...καὶ* correlative. This is against Krüger, who reads *δ' οὖν* for *τε οὖν*, and renders *καὶ* 'even.' Herbst may be right here; but his view that *δ' οὖν* is impossible because there is no antithesis with what precedes, is untenable. For there certainly is a sharp antithesis in the return from argument to the statement of facts which need no support. It is consoling to find two pages further on that one does not belong to 'alle Welt.' In c. 65 *sub fin.* *ποσοῦτον τῷ Περικλεῖ ἐπερίσσευσε τότε ἀφ' ὧν αὐτός [Classen 'und alle Welt' αὐτοῖς] προέγνω καὶ πάνν ἂν ῥαδίως περιγενέσθαι τῶν Πελοποννησίων*, the vulgate is clearly right as Herbst says; and I have retained *αὐτός* in my school edition. We have here the climax of Pericles' peculiar and penetrating foresight. But Herbst goes on to say that *αὐτός* is also subject to *περιγενέσθαι*, as though Thuc. considered Pericles as the one side and the Lacedaemonians as the other. Of course he supports this view by the passage about *τοῦ πρώτου ἀνδρὸς ἀρχή*. But it is very improbable that Thuc. would have gone so far as this; and very strongly against it is c. 65, 4 *ὁ μὲν γὰρ ἡσυχάζοντας...ἔφη περιέσεσθαι.* In our passage no subject is expressed to the infin., but surely 'the Athenians' is meant, as in i. 144 *πολλὰ καὶ ἄλλα ἔχω ἐς ἐλπίδα τοῦ περιέσεσθαι.*

In the same chapter, for *τρία μὲν ἔτη ἀντείχον* Herbst reads *<τρίς> τρία μὲν ἐ. ἂ.*, which is attractive but not convincing; for this is not a superstitious saying, as in the *τρίς ἐννέα ἔτη* of v. 26. Still in the same chapter, *αὐτοὶ ἐν σφίσι κατὰ τὰς ἰδίας διαφορὰς περιπεσόντες, ἐσφάλθησαν*, ἐν before *σφίσι* is bracketed, as Herwerden had done. This may be better perhaps than regarding *περιπεσόντες* as a gloss on *κατὰ τὰς ἰδίας διαφορὰς*, as I had done; but there is not much force in Herbst's contention that ἐν *σφίσι* could not be used here for ἐν *σφίσιν αὐτοῖς* (i.e. in the sense of ἐν *ἀλλήλοις*); for ἐν *σφίσι* is not emphatic. The emphasis lies much more on *αὐτοὶ* and *ἰδίας*, which are surely sufficient to render the meaning of *σφίσι* clear.

Herbst has some excellent and, at first sight, cogent arguments on the siege of Plataea. He defends *μέρος μὲν τι καταλιπόντες τοῦ στρατοπέδου* in ii. 78, which is

frequently bracketed, and says that *στρατόπεδον* means 'the army in camp,' part of which was left behind when the main part started building the walls; and for the custom he compares among other passages ii. 71 *καὶ καθίσας τὸν στρατὸν ἐμελλε δρῶσειν τὴν γῆν*. This explanation gives a better meaning than Classen can suggest to *τὸ στρατόπεδον ἐπὶ τὸ τείχος ὥρμησε* in iii. 22. But Herbst overlooks several difficulties. (1) Just below this passage come the words *καταλιπόντες φύλακας τοῦ ἡμίσεος τείχους . . . ἀνεχώρησαν τῷ στρατῷ*. If the *μέρος* *τι* τοῦ *στρατοπέδου* was left behind at Plataea as well as the garrison, as iii. 22 then requires us to assume, *ὁ στρατός* must here be used for only the main part of the army which had built the walls. But this is very obscure and unlikely. (2) Assuming that *στρατόπεδον* means here and in iii. 22 the army in camp (which Classen denies), do the words still yield the sense Herbst attaches to them? 'I leave behind part of the encamped-army, and proceed to build' is a very muddle-headed way of expressing 'I leave part of my forces in a camp and with the rest of my forces proceed to build.' In place of *τοῦ στρατοπέδου* we expect *ἐν τῷ στρατοπέδῳ*. Herbst says that Thuc. might have written *τῷ δὲ περιτείχιζον*; but this is no better, since *τοῦ στρατοπέδου* would still be 'understood' with *πλείονι*, and *στρατόπεδον* is to mean 'encamped-army' in opposition to the army engaged in building and garrisoning the walls! (3) Thuc. uses *κατα-*

*λείπω* thirty-four times, and in no other instance is there any ambiguity. The nearest parallel is iv. 127 *μέρος δέ τι καταλιπόντες οἱ λοιποὶ χωρήσαντες δρόμῳ*. In our passage one expects *καταλιπόντες* to mean 'left behind when the rest departed for home,' as the person saw who first imported the words *τὸ δὲ πλέον ἀφέντες*.

Perhaps, then, there is an error in *τοῦ στρατοπέδου*. It may be that Thuc. wrote *μέρος μὲν τι αὐτοῦ καταλιπόντες*, where *αὐτοῦ* was an adverb in the sense of *κατὰ χώραν*. This would very naturally be explained by *τοῦ στρατοπέδου*. But Herbst has certainly not cleared away the doubts surrounding this passage.

In c. 87 οὐχὶ *δικαίαν* *ἔχει τέκμαρσιν* *τὸ ἐκφοβῆσαι*, Herbst explains *τὸ ἐκφοβῆσαι* as direct object of *τέκμαρσιν*. If so, *τέκμαρσις* *τὸ ἐκφοβῆσαι* must be equivalent to *τὸ δικαίως τεκμαίρεσθαι τὸ ἐκφοβῆσαι*: but can any satisfactory sense be extracted from that? I note that Sitzler in his edition follows my suggestion *πεφοβῆσθαι* for *ἐκφοβῆσαι*, but reads *τοῦ* for *τό* (p. 227 of my school ed.). In c. 89 Herbst defends *εἵτα* *καὶ παρὰ ταῖς τε ναυσὶ μένοντες* of BAEFM, whereas CG omit *τε*. In c. 93 he proposes *ἀπὸ τοῦ ἀπροφανοῦς* in the terrible passage *ἐπεὶ οὐδ' ἀπὸ τοῦ προφανοῦς τολμήσαι ἂν καθ' ἡσυχίαν, οὐδ' εἰ διενοοῦντο, μὴ οὐκ ἂν προαισθῆσθαι*. But, even so, great difficulties remain for the next critic who shall have the courage to comment on this passage.

E. C. MARCHANT.

#### APELT'S PSEUDO-ARISTOTELIAN TREATISES.

(Continued from page 214.)

##### DE GORGIA.

187, 15. 979<sup>b</sup> 17. *ἀνάγκη γάρ, φησίν, εἴ τι ἔστι μῆτε ἐν μῆτε πολλὰ εἶναι, μῆτε ἀγέννητα μῆτε γενόμενα, οὐδὲν ἂν εἴη. εἰ γὰρ εἴη τι, τούτων ἂν θάτερα εἴη*. Bonitz supposes that between *εἴ τι ἔστι* and *μῆτε ἐν* the words 'ἦτοι ἐν ἡ πολλὰ εἶναι καὶ ἦτοι ἀγέννητα ἡ γενόμενα. εἰ δὲ ἐνυμβαίνει' have fallen out, and Apelt inserts them in his text. The addition makes good sense, but there is no homoeoteleuton to account for such a long omission which considerably exceeds the length of an average line; and besides the sense of the clause supplied is really given by the next sentence—*εἰ γὰρ εἴη τι, τούτων ἂν θάτερα εἴη*.

It seems more likely that an illative particle or phrase has fallen out before *οὐδὲν ἂν εἴη*: e.g. it would be enough to write *οὐδὲν <οὖν> ἂν εἴη*. Mullach writes *<τοῦτο δὲ> οὐδὲν ἂν εἴη*, but *οὐ <τω δὲ οὖ> δὲν ἂν εἴη* seems better.

188, 14. 979<sup>a</sup> 34. *οὐδαμῶθεν δὲ συμβαίνει ἐξ ὧν εἴρηκεν μηδὲν εἶναι. ἃ γὰρ καὶ ἀποδείκνυσιν, οὕτως διαλέγεται. εἰ (ἡ Lps.) τὸ μὴ ὄν ἔστιν ἡ ἔστιν (om. Lps.) ἀπλῶς εἰπεῖν εἴη καὶ ἔστιν ὁμοῖον μὴ ὄν. τοῦτο δὲ οὔτε φαίνεται οὕτως οὔτε ἀνάγκη, ἀλλ' ὥσπερ εἰ δυοῖν ὄντων τοῦ μὲν ὄντος τοῦ δ' οὐκ ὄντος τὸ μὲν ἔστι τὸ δ' οὐκ ἀληθές, οὔτε ἔστι τὸ μὲν μὴ ὄν. Apelt*







auf die Frage διὰ τι οὐκ ἔστιν κ.τ.λ. mit Berufung auf den bekannten Sprachgebrauch des Plato wonach τὸ δέ = quin imo.<sup>1</sup>

He supposes the sentence should be construed as if οὔτε were found before ἄμφο and remarks: 'es scheint zumal wenn das Verbum seine Negation ohnedies hat, der Ausfall des ersten οὔτε grammatisch möglich zu sein.'

The grammatical question is hardly clear. In the first place τὸ δέ in the sense of quin imo introduces something opposed to what has just preceded, and therefore it may be doubted whether it could possibly follow an interrogative clause like διὰ τί οὐν οὐκ ἔστι κ.τ.λ. which itself implies the opposition supposed to be conveyed in τὸ δ' ἄμφο κ.τ.λ. and is not therefore contrasted with it.

Secondly in the idiom where οὔτε appears to be omitted before the first of two notions each of which would normally be preceded by οὔτε, it seems natural that the first notion should be one which can stand by itself and does not necessarily imply the presence of the other, which is merely added on to it: and this is confirmed by such examples as are given in Kühner *Gr. Gr.* §536 l. with οὔτε and οὐδέ, e.g. Pind. *Pyth.* 6. 48 ἀδικον οὐθ' ὑπέροπλον ἦβαν δρέπων: Hdt. 1, 215 σιδήρω δὲ οὐδ' ἀργύρῳ χρέωνται οὐδέν.

But when the notions imply one another as alternatives which make up a whole, it seems hardly possible, in prose at least, that either of the conjunctions should be absent: and according to the editor's interpretation of the passage ἄμφο (both together) and ἕτερον (each by itself) would be alternatives of this kind, so that οὐτ' ἄμφο οὐθ' ἕτερον would seem necessary. Apelt appeals to Heindorf's note on Plat. *Parmenides* 152 E; but he does not take into account the difference recognised there between οὔτε and οὐδέ. In *Parmenides* 152 E τὸ ἐν ἅρα τὸν ἴσον χρόνον αὐτὸ ἐαυτῷ καὶ γινόμενον καὶ ὃν οὔτε νεώτερον οὔτε πρεσβύτερον ἐαυτοῦ ἔστιν οὔτε γίγνεται, it is true Heindorf thinks οὔτε need not be inserted before ἔστιν, but then he wishes to read οὐδέ γίγνεται instead of οὔτε γίγνεται, and this change is adopted both by Bekker and Stallbaum. As an instance of omission of the first negative Heindorf quotes *Parm.* 155 B οὔτε τὸ ἐν τῶν ἄλλων πρεσβύτερον γίγνοισ' ἂν οὐδέ νεώτερον, οὔτε τὰλλα τοῦ ἐνός ('ante πρεσβύτερον negandi vocula deest'), but here again the negative is οὐδέ and not οὔτε. Heindorf

also quotes *Parm.* 166 B ἐν ἅρα εἰ μὴ ἔστι, τὰλλα οὔτε ἔστιν οὔτε δοξάζεται ἐν οὔτε πολλά, without comment. But on the one hand the last οὔτε ought on his own principle to be changed to οὐδέ, and on the other hand Stallbaum does make the change in his text with the remark<sup>2</sup> 'emendandum esse οὐδέ πολλά vel praecedentia evincunt ἐν ἅρα εἰ μὴ ἔστιν οὐδέ δοξάζεται τι τῶν ἄλλων ἐν εἶναι οὐδέ πολλά. Ex codicibus emendata est quod 154 C vulgo legebatur πρεσβύτερον οὔτε νεώτερον: he reads οὐδέ in this last passage.

But the principal difficulty is in the sense which would result from the editor's interpretation. In any case the writer of the treatise is attacking the thesis of Gorgias οὔτε ἔστιν οὔτε εἶναι οὔτε μὴ εἶναι. If he denied one of the two propositions contained in this we should certainly expect him to deny the first (οὐκ ἔστιν εἶναι = τὸ ὃν οὐκ ἔστι), and to admit that in the proper sense of 'Being' the second (τὸ μὴ ὃν οὐκ ἔστι) was right enough, in accordance with what he himself has just argued against Gorgias—τὸ δ' οὐκ ἀληθές ὅτι ἔστι, τὸ μὲν μὴ ὃν (979<sup>b</sup> 1). But, on the editor's interpretation, in answer to the thesis he would not only point out that Gorgias himself had denied the second part of it, but would endorse this denial by the remark 'no one would say that there is no sense in which Not-being is,' meaning simply that Not-being *is* in the sense that 'Not-being *is* not-being.' And yet it is the object of the writer both in the preceding and following context to show that this latter identical proposition, which Gorgias maintains, is futile because nothing can be inferred from it about 'Being' in the proper sense. It is not likely that he would himself take advantage of this ambiguity instead of relying for his answer on the clear and unambiguous proposition τὸ ὃν ἔστι and denying the first part of the thesis instead of the second.

It may be added that οὐδαμῶς would be more naturally construed with φησί than with the infinitive depending on it, and that, if the editor were right, οὐδεὶς ἂν φήσκει would rather be expected than οὐδεὶς φησι. His rendering of the force of ὅτε ('und was das letztere anlangt') is rather a serious difficulty.

The text certainly seems very obscure. A suggestion may be made about its drift, though an accurate restoration of the original is not attempted. The natural objection to this particular paradox of Gorgias is that it obviously involves him in a contradiction, for in the argument here attributed

<sup>1</sup> From poetry might be quoted as a contrary instance *ναυαὶ δ' οὐτε πρὸς ἰὼν* Pind. *Pyth.* 10. 29 *cit.* Kühner *l.c.*

<sup>2</sup> See Stallbaum's note on *Parm.* 152 E.

to him he begins by contending that Not-being is, and ends with the inference that neither Being nor Not-being is. It is not likely that the writer of this treatise would fail to see the objection and to urge it.<sup>1</sup>

If we omit for the moment the sentence τὸ δ' ἄμφω κ.τ.λ. and read in 979<sup>b</sup> 4 ὅτε οὐδ' ἐν φησι for ὅτε οὐδεὶς φησι, the right kind of argument seems the result:—'Why then (according to Gorgias) are both Being and Not-being not existent? For he says that Not-being, if it is, as he thinks it is (εἴπερ), something [viz. is Not-being], has as much existence as Being, and yet in the same breath (ὅτε) denies that Not-being has any kind of Being,' ὅτε οὐδέν φησιν εἶναι τὸ μὴ εἶναι οὐδαμῶς would thus refer to the second half of the paradox οὐκ ἔστιν οὔτε εἶναι οὔτε μὴ εἶναι. Perhaps then the sentence τὸ δ' ἄμφω οὐθ' ἕτερον οὐκ ἔστιν, whatever may be its true form, is parenthetical, and meant to bring out the fact that Gorgias was committed to the two halves of the paradox separately as well as together, and therefore committed to the second though he had affirmed the contrary of it: *quasi* εἰ δ' ἄμφω οὐθ' ἕτερον οὐκ ἔστι, or τὸ δ' ἄμφω οὐκ ἔστιν οὐδ' ἕτερον οὐκ ἔστι= 'both are not' is the same as saying 'each of them is not.'

In this way φησὶν refers to Gorgias as it does in this context throughout, and the difficulty, already noticed, of the substitution of φησί for φήσκειν ἄν would be avoided.

189, 2. 979<sup>b</sup> 5. εἰ δὲ καὶ ἔστι τὸ μὴ ὄν μὴ ὄν, οὐχ οὕτως ὁμοίως εἶη ἂν τὸ μὴ ὄν τῷ ὄντι· τὸ μὲν γάρ ἐστι μὴ ὄν, τὸ δὲ καὶ ἔστιν ἔτι. εἰ δὲ καὶ ἀπλῶς εἰπείν ἀληθές, ὥς δὴ θανμασίον γ' ἂν εἴη τὸ μὴ ὄν ἔστιν.

τὸ δὲ καὶ ἔστιν ἔτι seems hardly adequate to express what is intended. One would expect τὸ δὲ καὶ ἀπλῶς ἔστιν. ἔτι δ' εἰ καὶ

<sup>1</sup> Apparently Apelt also supposes that it is intended in the text to convict Gorgias of a contradiction (cp. 'Gorgias sagt ja selbst'): but he does not make the clause ὅτε κ.τ.λ. serve to convey the contradiction, though the particle ὅτε suggests that such is its function. But if we follow the MSS., as Apelt does, and read ὅτε οὐδεὶς, the proper interpretation of the clause is surely 'though no one thinks that Not-Being has any kind of being.' This would be a refutation of εἴπερ εἶη τι καὶ τὸ μὴ εἶναι, and thus the argument would not be that Gorgias contradicted himself, but that he contradicted an established truth. The objections to acquiescing in this last interpretation are: (1) while the self-contradiction in Gorgias would not be pointed out at all, (2) a redundant argument would be produced, since pretty much the same thing is said in the next sentence, 979<sup>b</sup> 8, and before in 979<sup>b</sup> 1; and (3) the writer would not be likely to say οὐδαμῶς, for he would not disallow the proposition τὸ μὴ ὄν ἔστι μὴ ὄν though he disputes the inference from it.

ἀπλῶς κ.τ.λ. Perhaps ἔστιν was lost before ἔτι, and then the correction ἔστι written above displaced ἀπλῶς. Or possibly the original was τὸ δὲ καὶ ἀπλῶς ἔστιν. εἰ δὲ καὶ and ἔστι being corrupted to ἔτι had ἔστι written over it, the superscript displacing ἀπλῶς as before. For εἴη τὸ should perhaps be read εἴη εἰ τὸ.

189, 6. 979<sup>b</sup> 8. 'πότερον μᾶλλον ξυμβαίνει ἅπαντα ἢ εἶναι μὴ εἶναι Lps.; εἶναι ἢ μὴ εἶναι ceteri. Quae Lps. habet, sententiae satisfaciunt, sed commodius verba sic collocantur: μὴ εἶναι ἢ εἶναι et sic forte scribendum' (Apelt). But surely the order in Lps. is not possible, and μὴ εἶναι ἢ εἶναι should be restored.

190, 13. 979<sup>b</sup> 31. οὐδὲν ἂν ἐκ μηδενὸς ἂν γενέσθαι. 'alterum ἂν del. Mullach, sed forte scribendum οὐδὲ τὸ ὄν ἐκ μηδενὸς ἂν γενέσθαι.' The repetition should cause no difficulty. See above on 976<sup>a</sup> 3. Mullach also suppresses the second ἂν in 976<sup>a</sup> 30, πῶς ἂν ἀπειρον ἂν εἴη, where it may be right.

190, 17. 979<sup>b</sup> 35. ἔτι εἴπερ ἔστι τι, ἢ ἐν ἡ πλείω, φησὶν, ἔστιν· εἰ δὲ μήτε ἐν μήτε πολλά, οὐδὲν ἂν εἴη. καὶ ἐν μὲν...καὶ ὅτι ἀσώματον ἂν εἴητο...ἐν κ...ἔσχον (vel εἴχον) μὲν γε...τῷ τοῦ Ζήνωνος λόγῳ. ἐνὸς δὲ ὄντος οὐδ' ἂν...εἶναι. οὐδὲ μὴ...μήτε πολλά...εἰ δὲ μήτε...μήτε πολλά ἔστιν, οὐδὲν ἔστιν. Lps. Other MSS. have εἴη τὸ ἐν ἡ ἐνσχυομένη γε without mark of lacuna here, and εἰ γὰρ μήτε ἐν μήτε πολλά ἔστιν without lacuna, except R<sup>a</sup> which has a lacuna after γάρ.

Foss has proposed the following emendation:—

καὶ ἐν μὲν <οὐκ ἂν δύνασθαι εἶ>ναι ὅτι ἀσώματον ἂν εἴη τὸ ἐν· <τὸ γὰρ ἀσώματόν φ>η<σιν οὐδ>ἐν ἔχον μὲν γε <θ>ος· ὁ ἀναιρείσθαι> τῷ τοῦ Ζήνωνος λόγῳ. ἐνὸς δὲ <μὴ> ὄντος, οὐδ' ἂν <πολλά> εἶναι, οὐδὲ μὴ<ν εἰ τι> μήτε πολλά <μήτε ἐν ἔστιν, εἶναι ἂν οὐδαμῶς, εἰ δ' οὕτως φησὶν, οὐδὲν ἔστιν,> εἰ γὰρ μήτε ἐν μήτε πολλά ἔστιν οὐδὲν ἔστιν.

Apelt reads: καὶ ἐν μὲν <οὐκ ἂν εἶ>ναι, ὅτι ἀσώματον ἂν εἴη τὸ <ὡς ἀληθῶς> ἐν, κ<αθ>ο οὐδ>ἐν ἔχον μὲν γε <θ>ος· ὁ ἀναιρείσθαι> τῷ τοῦ Ζήνωνος λόγῳ. ἐνὸς δὲ <μὴ> ὄντος, οὐδ' ἂν <ὅλως> εἶναι οὐδέ. μὴ <γὰρ ὄντος ἐνός> μὴδὲ πολλά <εἶναι δεῖν>. εἰ δὲ μήτε <ἐν, φησιν>, μήτε πολλά ἔστιν, οὐδὲν ἔστιν.

Both give what seems likely to be the general sense of the original. Apelt's emendations are nearer the Greek, and agree better with the lengths of the lacunae as indicated in Lps.: μέγεθος is a clever conjecture. The argument however seems to require for its completion a clause like τὸ δ' ἀσώματον οὐδέν, thus:—'Being cannot be one, because the true One is incorporeal (which he proves like Zeno), and that which is

incorporeal is nothing.' This clause appears in Foss's emendation, but is wanting in Apelt's. On the other hand the words *ἐχόμενός γε τοῦ τοῦ Ζήνωνος λόγου* are awkwardly placed in Foss's version, for they do not refer to what immediately precedes them but to the clause *ὅτι ἀσώματον ἂν εἴη τὸ ὄν*: *ἀναρτῆσθαι*, conjectured by Apelt, seems hardly the right word.

Partly following Foss and Apelt one might suggest something like this—*καὶ ἐν μὲν <οὐκ ἂν εἴ>ναι ὅτι ἀσώματον ἂν εἴη τὸ <ἐν, τὸ δ' ἀσώματον οὐδ' >έν. κ<αὶ τὸ> ἐν οὐκ ὁ ν μέγε<θος λαμβάνει> τῷ τοῦ Ζήνωνος λόγῳ.*

This would avoid the objections named, but is not quite satisfactory. If the general form of Apelt's emendation is preferred it would surely be better to write *καθὼς οὐκ ἔχον μ ε ρ η*, for *κ<αθὼς οὐδ' >ἐν ἔχον μ ε ρ η <ε<θος>* is rather tautologous when offered as a reason for *ἀσώματον ἂν εἴη*, whereas *μέρη* exactly suits Zeno's reasoning,<sup>1</sup> here referred to, and seems a possible corruption of *μέν γε*.

But the argument which seems clearly enough implied in the fragments of the text requires for its adequate expression hardly less than the following:—*καὶ ἐν μὲν <οὐκ ἂν εἴ>ναι ὅτι ἀσώματον ἂν εἴη τὸ <ἐν, τὸ δ' ἀσώματον οὐδ' >έν, κ<αὶ ἀσώματον μὲν τὸ> ἐν ὥς οὐκ ἔχον μ ε ρ η <λαμβάνει> τῷ τοῦ Ζήνωνος λόγῳ.* The text is probably too mutilated to give material for an exact restoration.

190, 8. 979<sup>b</sup> 27. *γενέσθαι γοῦν οὐδὲν ἂν οὐτ' ἐξ ὄντος οὐτ' ἐκ μὴ ὄντος. εἰ γὰρ τὸ ὄν μεταπέσοι, οὐκ ἂν ἔτ' εἶναι αὐτὸ ὄν, ὥσπερ γ' εἰ καὶ τὸ μὴ ὄν γένοιτο οὐκ ἂν ἔτι εἴη μὴ ὄν. οὐδὲ μὴν οὐκ ἐξ ὄντος ἂν γενέσθαι κ.τ.λ.*

Bonitz, following Foss, found a difficulty in the substitution of *μεταπεσεῖν* for *γενέσθαι* without explanation, and suggested *εἰ γὰρ τὸ ὄν <γένοιτο μεταπέσειεν ἂν εἰ δὲ τὸ ὄν>μεταπέσοι*. Apelt reads *εἰ γὰρ <ἐξ ὄντος γένοιτο μεταπεσεῖν ἂν, ὃ ἀδύνατον εἰ γὰρ> τὸ ὄν μεταπέσοι*.

There is something corresponding to this passage in one of the fragments of Melissus (17 Mullach) *ἦν δὲ μεταπέση τὸ μὲν ὄν ἀπώλετο, τὸ δ' οὐκ ὄν γέγονε*, and in the fragment *μεταπεσεῖν* either is equivalent to *γενέσθαι*, or else *μεταπεσεῖν* means change in something previously existent and *γενέσθαι* rather the coming into being of that which did not exist at all, a distinction which exactly suits the passage before us as it stands in the MSS. Bonitz's emendation therefore seems faulty because it makes *μεταπεσεῖν* a consequence of *γενέσθαι*.

Apelt's emendation is much better, but as

<sup>1</sup> Cf. 976<sup>b</sup> 5.

the sense of the passage must be that if something came out of *τὸ ὄν* a change would thereby take place in *τὸ ὄν*, we should rather read *εἰ γὰρ ἐξ ὄντος γένοιτο μεταπεσεῖν ἂν τὸ ὄν*, for the subject of *μεταπεσεῖν* is not the same as that of *γένοιτο*.

But is it really likely that anything has been lost? The text as it stands would imply what Apelt proposes to add, though it is true the addition makes it clearer. Indeed the next clause, which relates to *τὸ μὴ ὄν*, is of exactly the same form as the clause about *τὸ ὄν* which it is proposed to emend, and implies the same argument also. No one has suspected that anything is lost in it, yet if the first clause is emended, this also might be emended on the same principle, thus:—*ὥσπερ γ' εἰ καὶ <ἐκ μὴ ὄντος γένοιτο, γένοιτ' ἂν τὸ μὴ ὄν, ὃ ἀδύνατον, εἰ γὰρ> τὸ μὴ ὄν γένοιτο οὐκ ἂν ἔτι εἴη μὴ ὄν.* It may of course be contended that the second clause does not necessarily imply the same argument as the first and that the comparison is merely between *εἰ τὸ ὄν μεταπέσοι οὐκ ἂν ἔτ' εἶναι αὐτὸ ὄν* and *εἰ τὸ μὴ ὄν γένοιτο οὐκ ἂν ἔτι εἴη μὴ ὄν*—'if Being were to change it would not be Being, just as if Not-being were to change it would not be Not-being.' But the comparison would be weak and without point. The idea of the change of Not-being is artificial and certainly at least not one by a comparison with which (*ὥσπερ*) the change of Being could be elucidated. The idea only arises as a consequence of the hypothesis that something comes out of Nothing or Not-being, and it is here alone that the comparison has any meaning. 'Generation out of Being involves change of Being which is impossible, because if Being changed it would cease to be Being; just as also generation out of Not-being involves change in Not-being, which is impossible, for then Not-being would cease to be Not-being.'

The fact also that the two clauses have the same form in the original is of course much in favour of interpreting them in the same way. If then the text be left as it is in the MSS. the interpretation would be as follows:—'Nothing could be generated out of Being or out of Not-Being. (For this would involve a change either in Being or Not-being.) But if Being changed it would cease to be Being, just as if Not-being changed it would no longer be Not-being.'

It is certainly an objection to this defence of the text that the next sentence again treats of the case of generation from Not-being and in its last clause implies, though it does not express, the argument above given—*δι' ἅπερ οὐδ' ἐκ τοῦ ὄντος διὰ ταῦτα οὐδ'*

ἐκ τοῦ μὴ ὅπως γενέσθαι. But here the redundancy is not greater than the style of the treatise seems to allow, whether it be the fault of the original writer or has come through addition to his work. Cf. 979<sup>a</sup> *fin.* with 979<sup>b</sup> 5—7, the argument in 977<sup>a</sup> 23

*sqq.*, already discussed, and other places. There is therefore perhaps not sufficient reason for altering a text which does not show any obvious trace of corruption such as *e.g.* unsound grammatical structure.

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(To be continued.)

#### MENAECHMI OF PLAUTUS, BY BRIX AND NIEMEYER.

*The Menaechmi of Plautus*, edited for school use by JULIUS BRIX; 4th edition revised by MAX NIEMEYER (Teubner, Leipzig, 1891). 1 Mk.

THE merits of Brix's editions of several plays of Plautus are well known; few men have done more than he to advance the study of Plautus, both by his own accurate researches into problems of language and metre, and by making generally known the results of others' work. On the lamented death of Brix in 1887 at the age of seventy-two years the further editing of his plays was entrusted to Dr. Max Niemeyer of Potsdam, the author of several short treatises on Plautine questions. In this volume of the *Menaechmi* Dr. Niemeyer speaks very modestly of his own work on the text: it was his object to produce a *readable* (i.e. construable) rather than a scientifically sound text, and he has therefore admitted 'conjectures which he would not have considered worth even a mention in a critical edition.' But it would be a mistake to infer that the text is a purely fanciful one; on the contrary it is evident throughout that Dr. Niemeyer is not indifferent to critical questions and that he has had an eye to the results of recent work on the *Menaechmi*. In many passages one finds improvements on the text and notes of Brix's third edition: *e.g.* 75 Schoell's *habitat* is rightly introduced: so too Wagner's dactyls in 114.—141 *facinus luculentum* is better explained than by Brix.—156 the explanation which occurred independently to Schoell and to F. D. Allen (*solum* = eye-socket) is rightly adopted.—166 Ussing's punctuation *pallam, quid olet* is good.—168 *inlubitili* of Nonius has much to commend it.—200 Schoell's PE. is rightly introduced.—238 I am glad to see that Niemeyer has here and throughout the play abolished the spellings *sei* for *si*, etc.; they happen to

be very numerous in the MSS. of the *Menaechmi* and if admitted would disfigure many passages.—281 the quantity *ubi* is rightly admitted; the note however should have quoted instances like Cure. 340 (MSS.), Aul. 700 (MSS.), Truc. 506 (MSS.), cf. *ibi* Men. 187, *ibidem* Rud. 1236, *mihi, tibi*, etc.—361 I am glad to see the last of *homonem*; but I still believe the true reading is *Heu hercle hominem* (with hiatus), as I said in my review of Schoell's edition (*Class. Rev.* IV. p. 213): cf. *flagitium hominis* 709, 489 and 407 (where Niemeyer recognizes hiatus before *hom.*): see below on 713.—387 *tam gratias* is better explained than by Brix.—480 *atque eam meas* (MSS.) is probably all right in view of the facts quoted by Klotz, 'Grundzüge der altrömischen Metrik,' p. 244 ff.—495 *homini hic ignoto insciens* is rightly adopted (with Schoell) from Camerarius and B<sup>2</sup>.—541 f. *da mihi faciundas* (MSS.) is rightly restored (with Ussing); but there might be a note that it means 'get made for me': cf. 733.—561 *ea* (for *hinc*) is well restored from A, according to the reading of Studemund.—569 the omission of the name MA. is rightly adopted from Seyffert.—571 ff. the adoption of Schoell's way of printing the series of bacchiacs is an improvement.—613 *comesses* is better than *comessis*.—681 *Tibi equidem dedi* is better than *Tibi dedi equidem*; but reading so Niemeyer ought to have abolished the note on hiatus in the diaeresis.—849 Vahlen's excellent reading *Ni & 1 meis oculis abscedat* is rightly adopted (Schoell's objections were shown by Seyffert to be unfounded); but I cannot believe in the order of words at the end of the line *in malam magnam crucem* (MSS.). Klotz, who does not seem to be aware of Vahlen's reading, defends the MS. order (p. 234); but he does not quote any other instance to justify the separation of *malam* from *crucem*: the group *mala crux* expresses a single idea,

<sup>1</sup> Better *Ni a*.



and if it is to be qualified the qualifying adjective cannot come in the middle. Read *in magnam malām crucem*: the two iambic words at the end of the line are legitimate in this phrase, and the spondaic word *magnam* in this place can be easily justified.

On the other hand I regret to see some changes which I consider to be for the worse. One cannot but wish that Niemeyer had confined himself to the minimum of necessary change, and so left to the edition of Brix as far as possible its characteristic features. For example I doubt whether it improves the sense to attribute line 185 to Peniculus. —188 Niemeyer's conjecture *cum uiro* will hardly be accepted; Schoell's *cum eo* would be more 'readable.' —249 *Dictum facessas* 'a truce to your wit' is not supported by the other passages in which *facessere* occurs in Plaut.; *hinc* is absolutely required before *facessas* in the sense 'away with' as in Rud. 1062; without *hinc* it would have the sense which it has in Rud. 1061. —277 I doubt the possibility of the accentuation *priūs* at the beginning of the line. —312 *sani's* is better than *sanus*. —407 *Nesciō quem* is surely doubtful in view of the fact that the Plautine accentuation is either *nēsciōquis* or *nesciōquis*: Brix inserted *tu*. Klotz in his 'Grundzüge' p. 51 does not bring evidence sufficient to establish *nesciō quis*, though *nesciō* in other connexions is quite admissible, e.g. Men. 530). —432 Vahlen's *sussuli* is not suitable to the context; the whole passage has yet to be cleared up. —460 *datum voluisse* is not really suitable to the context; why not *data verba esse* if we are to be readable? —487 the note on *ais* requires correction; it is sometimes two syllables, but far more often three, as in the common formula *sed quid ais?* —536 *istuc* in the sense in which Vahlen understands it requires illustration and proof from Plautus. —556 I do not believe that *ut si quis sequatur* can be scanned as — — — — —; but I am by no means sure that Brix's reading based on Nonius is right. —602 is not a correct senarius. —626 f. the introduction of PE. and MA. speaking in chorus is doubtful. —680 the emendation of Brix (*quom* for *quam*) ought not to have been rejected: Pers. 153 is not to the point in regard to the last part of the line. —740 Has Vahlen's *aufers* really any probability? The line should at any rate be better punctuated. —809 *discertatis* (Brix, supported by D) is more vigorous than *dissertatis*. —846 *censeo* is questionable.

I append a few miscellaneous remarks as contributions towards a future edition. Line 23 should have a colon after *uidi*. —65 a

note is wanted on the omission of the subject. —89 *deliges* is jussive, not potential. —98 *illie* must be scanned with the first syllable long (reading *hercle*). —103 the subjunctive *petas* might be discussed; cf. my note on Rud. 1021 or that on 1329 (*quo addas*): on the latter method of explaining *petas* would be like Men. 502 *facias* (*si facias* 'if you are to do'; I doubt Niemeyer's treatment here). —144 a note is wanted on the subjunctive *raperet*, cf. Rud. 129, 315, 320. —146 the note on *istic* is not quite clear, and hardly consistent, as it stands, with that on 98. —160 *esses agitator probus*: not, as N. says, 'du würdest sein (wenn du in den Fall kämest)' with reference to future time, but either with reference to present time 'you would be excellent as a circus driver' (= *si agitator esses*, cf. Cas. 811), or with reference to past time 'you would have been' etc. (= *fuisses*, Mil. 112). —178 a note is wanted on *mille passum commoratus cantharum*. —195 *si amabas* requires explanation; and Brix and Niemeyer's translation 'wenn du ihn wirklich liebtest' is ambiguous or misleading ('wirklich'). If *si amabas* 'if you loved him,' refers to present time, we have a strange use of the imperf. indic. (= *amares*); if it refers to past time, as in Pseud. 286, Rud. 379 f., etc., the fact ought to be stated; and *oportebat* then also requires a note. —205 *anno* should stand after *emi*, as in the MSS., including A. —258 a note might be added on the two forms *Epidamnus*, *Epidamniensis* (32, 57, 1000). —362 a comma at *pateant* would be an improvement. —416 note wanted on *quin* with imperative. —428 *eadem opera ignorabitur* (Weise, Fleckeisen) I consider far better than either Brix's *et eadem ign.* or Niemeyer's *eadem enim ign.*; the subject of *ign.* is *palla* (understood), cf. 468; and the *ne*-clause is final, Schoell's objection to *ignorabitur ne* thus falling to the ground: the fact that *opera* stands in the previous line (in a different sense) is no argument against the emendation, but rather may be used to support it. —433 the note would be clearer if it ran: *ut 'hew' = 'what.'* —495 I would ask Niemeyer to suppress the note in the critical appendix; let us leave personalities to the politicians. —In the note on 589 *neque haud* should be mentioned, with a reference to 371. —592 f. is certainly not in order as printed by Niemeyer. —605 the note that *potis* stands 'as infinitive' is misleading; *potis* is an indeclinable adjective. —704 Niemeyer might borrow a remark on the present with *quam mox* from my note on Rud. 342. —713 I believe that it is unnecessary to insert *o*; Seyffert has called atten-



tion to the fact that we need not be afraid of apparent hiatus before the word *homo*: cf. above on 316.—717 Brix's note on *quemquem*, *quemque* requires to be entirely recast; it is based on a confusion of relative with indefinite meaning; in passages like Men. 522 *ut quemque conspicor*, Capt. 501 *ubi quisque vident*, Mil. 1264, Pseud. 1312, Rud. 1359 etc. *quisque* has its ordinary meaning 'each'; but in passages like Mil. 156, 160 *quemque videritis*, 460 *quemque videro* (add 1391 *quaeque aspexerit*), Capt. 797 f. *ad quemque icero* the word is relative (= *quisquis*). It is very strange that the attention of Brix himself was never directed to this note which he allowed to stand in three editions.<sup>1</sup>—736 I cannot agree with Brix that *quaero*=*quaeso* in *quaere meum patrem, tecum simul ut veniat ad me*; the meaning is simply 'seek (try to find) my father, in order that he may come with you'; whether

<sup>1</sup> Is it evidence that even eminent scholars are not always thoroughly alive to the essential point of difference in meaning between *quisquis*, *quicumque* on the one hand and *quisque*, *quivis*, *quisquam* on the other? That the latter in their ordinary use stand in sharp contrast to the former as non-relatives is a point generally obscured by grammars.

Langen so understands it ('Beiträge' p. 291, not 736) is perhaps not quite clear.—744 The passage quoted in the critical appendix to justify the indic. *arbitrare* is not to the point; the question is whether the indic. can stand in dependence on a *verbum nesciendi*; cf. my note on Rud. 385.—763 should have no comma at *mihi*.—764 there seems to be no reason to prefer *arcessat* to *accersit* (MSS.); see my notes on Rud. 1056 and 356.—896 the reading *quin suspirabo plus aescentos in dies* is almost certainly wrong.—913 *ellebori unguine* is quite unsuitable, as Schoell says; hellebore was drunk, not made into ointments. The MSS. have *ingere*: I propose to read *ingero* 'an acre of hellebore.'

Some misprints will have to be corrected: 172 *Elocutâ's* (with ictus); 185 *Vter* (without ictus); 224 *cura* (ditto); 255 *hercle* (ditto); p. 37 the note on 323 refers to 303, instead of 302; 396 *lubidrio*; p. 53 the numeration of four notes is wrong; p. 54 in note on 571 'bis 557' for 'bis 577'; 576 *siet'* for *si est* or *sist*; 584 *periuiriis*; 621 *Qu* (about ictus); p. 107 in margin 624 (for 625), 734 (for 739), 735 (for 740).

E. A. SONNENSCHNEIN.

#### PLASBERG ON THE *HORTENSIVS* OF CICERO.

*De M. Tullii Ciceronis Hortensio dialogo* scripsit OTTO PLASBERG. Lipsiae: Gustavus Fock. 1892.

It is a frequent source of jokes against anatomists that they are credited with an ability to construct an organism, given a tooth or some equally minute portion. The re-construction of lost works of literature from a few fragments, which are preserved in dictionaries or grammars, may lend itself to similar merriment: but happily not so in the present case, as the fragments of the *Hortensius* are fairly numerous and direct evidence is forthcoming on some points relating to the main features of the dialogue. The great literary and moral merits of the essay, which effected the conversion of a St. Augustine, justify every attempt to discover all that can be known about it. There is indeed no reason to be altogether without hope that it may be still discovered in some library: but meanwhile we must thank Herr Plasberg for the careful thought he has bestowed on the extant fragments, the learned and on

the whole satisfactory sketch he has made of what was probably the course of the dialogue, and the admirable manner in which he has fitted the fragments into the framework which he has constructed.

Perhaps the most important point in the pamphlet is the confutation of Schenkel's view that the *Hortensius* was unknown in the Middle Ages. There is no doubt that many allusions to a dialogue *Hortensius* really refer to the *Lucullus*: but in two instances at least Herr Plasberg shows that this is not the case. Bertholdus in his 'Annals' under the year 1054 (Pertz V. 268) relates a vision of Herimann, Abbot of Mehrerau, which proves that the latter must have read and re-read a *Hortensius*, and from what is stated of the contents of the book it is certain that it was not the *Lucullus* and was the *Hortensius*. Again, from some scholia on a Cambridge MS. of the *Academica* noticed by Dr. Reid (*Acad. ed.* 2, pp. 67—8), William of Malmesbury (circ. 1140) did not confound the *Hortensius* and the *Lucullus*. Hence too the practical result that when we find a *Hortensius* of

Cicero in any catalogue of MSS. we should not hastily say that it must be the *Lucullus*. The dialogue was composed in April 709 during Cicero's stay with Atticus in the country seat at Nomentum; hence there is little or no reference to the *Hortensius* in the *Letters to Atticus*. It is supposed to have taken place in the house of Lucullus at some time between 689 (the date of Cicero's speech for Cornelius) and 694 (the date of the death of Catulus). The interlocutors were Catulus, Lucullus, Hortensius and Cicero. Passing from admiration of the villa of Lucullus and its works of art to the consideration of books as more conducive to real education and culture, Catulus treats of the poets and Lucullus of historians. Hortensius then delivers a panegyric on the one study that is really valuable—eloquence, with allusion to many of the chief orators. This is followed by a discussion between Catulus and him, the former defending philosophy, the latter attacking it on the ground of its abstruseness and difficulty (formal logic he especially despises), the ambiguity that hangs round all its tenets, the diversity of views held by the most eminent thinkers, and the little practical effect it has had on the moral lives of the philosophers. Hortensius appears to think that philosophy, or the love of wisdom, should be directed to procuring elegance and refinement in all the material adjuncts of life; and he maintains these views with an acrimonious energy similar to that with which Callicles in the *Gorgias* maintains somewhat similar 'common-sense' opinions. Indeed it is not altogether impossible that Cicero may have had that declamation of Callicles before his mind (cp. Frag. 33 Btr. *Quae est igitur philosophia, Socrate?*), as the fragments show (e.g. 9, 74, 90) reminiscences of Plato. Cicero replies that much of the hostility to philosophy is due to insufficient preparatory training, both moral and intellectual, on the part of those who attempt to study it: and then proceeds in a long oration to prove the necessity of the study of philosophy. We all desire to be happy, but are led astray by false notions that happiness lies in glory and riches, by the allurements of pleasure and by the fear of death. Philosophy clears away these erroneous ideas, frees us from the tyranny of passion, proves to us the worthlessness of the body, and gives us good hopes that after death we shall enjoy either the sleep of extinction or the bliss of immortality.

In the details Herr Plasberg is often instructive. His reference of Frag. 37

*bellum cum mortuo gerunt* to the literary war which was raging round the memory of Cato is highly probable. We think he labours in vain (p. 25) to lay down rules for the (sporadic) use of the plural with *utrumque* in Cicero. He argues with considerable probability that in the *Lucullus* §§ 13, 144 (*seditioni cives, tribuni*) the reference is to Cornelius the tribune whom Cicero defended in 689; and with great learning he explains Frag. 17 *se ad extremum pollicetur prolaturum quae se ipsa comest quod efficit dialecticorum ratio* by a quotation from Julianus Pelagianus (cp. St. August. x. 726 ed. Migne) *non igitur sum pharmacopolae similis, ut dicis, qui promittebat bestiam quae se ipsam comesset* i.e. the polypus, cp. Hesiod *Op.* 522 and Lucilius 1042 ed. Lachm. He very fairly transfers Frag. 32, which is usually assigned to the *Hortensius*, to the beginning of the 3rd book *De Republica*. In criticism his conservatism is most laudable. Thus in Frag. 82 *quod alterius* (probably a comic poet) *ingenium sic ut acetum Aegyptium, alterius sic acre ut mel Hymettium dicimus* he justly refuses to transpose *acre* to the previous clause and substitute *dulce* or *mite*, cp., with Baiter, St. Augustine *De Vita Beata* ii. 14 (= i. 966 ed. Migne), *ut ait ille de melle Hymettio; acriter dulce est nihilque inflat viscera*. He refuses to alter *ut Cicero* in St. Augustine *Principia dialecticae* c. 6 (= i. 1412 ed. Migne) *Stoici autumant, quos Cicero in hac re ut Cicero inridet nullum esse verbum cuius non certa explicari origo possit*, though Haupt's *cevitros* is most attractive. He justly defends *talibus* in *litteris talibusque doctrinis* (Frag. 9) by reference to *Tusc.* i. 71, *De Orat.* ii. 185; *factus* without any addition such as *sublimis* in Frag. 44, *in interitu Romuli qui obscurations solis est factus*: in Frag. 88 *aptissime* (wrongly altered to *artissime*) by the apt quotation from Val. Max. ix. 2 extr. 10. However he need not have added *ista* in Frag. 33 *Quae est igitur philosophia, Socrate?* We should suggest to read *horum* for *eorum* in Frag. 32 to mark the antithesis to *istorum*; and perhaps Frag. 27 might be altered to *quantum inter se homines stud <ius dissid> entes moribus omnis vitae ratione differant*.

He breaks a lance with Dr. Reid over *Acad.* ii. 61 *Tunc, cum tantis laudibus philosophiam extuleris Hortensiumque nostrum dissentientem commoveris, eam philosophiam sequere* &c., where he expresses agreement with the view held by Krische that the reference is to the conversion of Hortensius at the close of our dialogue. This may be true: but he is not quite fair to Dr. Reid,

who considers (p. 44) that Hortensius in the *Catulus* gave a *résumé* of the history of philosophy in order 'to show that the New Academic revolt against the old Academico-Peripatetic school, as viewed by Antiochus, was unjustifiable.' Herr Plasberg does not consider the point of view from which the

*résumé* was made (si Hortensius in *Catulo* nihil protulit nisi quaedam de memoria philosophorum decerpta—id quod statuit Reidius), and then asks, How can Hortensius be said to have been moved from his position of dissent?

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#### SONNTAG'S VIRGIL AS A BUCOLIC POET.

*Vergil als bukolischer Dichter*, von M. SONNTAG, Oberlehrer am königl. Friedrichs-Gymnasium zu Frankfurt a/Oder. Leipzig. B. G. Teubner. 1891. Pp. 249. 5 Mk.

THE writer combats with great ability the commonly accepted view of the date and order of the Eclogues. 'The placing of the first Eclogue in the year B.C. 41 is the original mistake of all attempts at arrangement hitherto made.' The process of settling troops as colonists and the surveying and division of the territory assigned them, involving as it did the setting apart of *loci publica, pascua, silvae, loca sacra* and such properties as for some special reason were left in the possession of their original owners (*fundi concessi*), is shown by careful consideration of authorities to have been a long and tedious one. Even in the republican period, when the numbers to be settled were comparatively small, *triumviri coloniae deducendae* were appointed with authority for three years (p. 35), which may therefore be considered the average time necessary for the completion of their task. Now Mantua was not included in the territory originally granted to the veterans, and it was only after the territory of Cremona had proved insufficient (*i.e.* after it had been carefully surveyed and the various *exempta* and *concessa* determined), and consequently at a comparatively late period, that the trouble about Virgil's farm could have arisen. The visit to Rome which he took in consequence was made in the autumn (cf. *Ecl.* 1, 37, where Amaryllis leaves the ripe apples hanging in his absence), and the Eclogue represents a *spring* scene (cf. l. 14), so that its earliest possible date is B.C. 40. But it is impossible, for the reasons given above, that by the autumn of B.C. 41 the settlement of the veterans could have advanced so far that the occupation of Mantuan territory had by then commenced. More probably during the troubles of B.C. 41 and the early part of 40 things went on

very slowly and no real progress was made until after the peace of Brundisium, so that the appearance of the *agrimensores* at Mantua may be placed in the spring of B.C. 39, and Virgil's visit to Rome in the autumn of that year. At that time both Octavian and Pollio were in Rome, and we know on the authority of Servius, the *Scholia Bernensia* and the *Scholia Danielina*, that it was Pollio who introduced the poet to Octavian, an introduction which could not have taken place in B.C. 41 when he was distinctly opposed to Octavian, but which was perfectly easy in B.C. 39 when he was in high favour and had just returned from his victory over the Parthini. The date of the first Eclogue is thus brought down to the spring of B.C. 38.

Further, accepting a note of the *Scholia Danielina*, which on *Ecl.* 9, 10 explains *carminibus* by a reference to the *carmina quibus sibi Pollionem intercessorem apud Augustum conciliaverat*, and *Ecl.* 8, 11, *accipe iussis carmina coepta tuis*, taking the plural *carmina* strictly and not merely as referring to the single Eclogue, the writer asks what were the 'poems' of which Virgil thus asks Pollio's acceptance, which Pollio had urged him to write and of which the eighth Eclogue is the last (cf. *a te principium tibi desinam*). First come the second and third Eclogues, which are purely experimental imitations of Theocritus; with these goes the fifth Eclogue which refers to them and is also purely Theocritean, the view of Nettlehip that Daphnis represents Caesar being strongly opposed (p. 122); the fourth is necessarily included and is an attempt to raise bucolic poetry to a higher level (l. 1 *maiora canamus*) in accordance with a suggestion of Pollio's, who found the second and third somewhat lacking in elevation (cf. *non omnes arbusta iuvant humilesque myricae*); the seventh completes the collection, and the eighth though written last was placed at its head. It was this collection which Virgil presented for

Pollio's acceptance when he came to Rome in the autumn of B.C. 39 to appeal for his assistance. The words *tu mihi* (*Ecl.* 8, 5) introduce that appeal, which is perfectly clear though made indirectly; when Virgil begins 'do thou, I pray' and then goes on 'O shall I ever be allowed to sing thy fame?' he means 'do thou, I pray, make it possible for me to pursue my poetic task,' or in plain words 'get me back my farm.'

To this original collection were subsequently added Eclogues 1, 6, and 9, all of which are directly connected with Virgil's trouble about his farm, and the tenth which is expressly stated to be written last; the present first Eclogue was then placed in its present position, thus dedicating the complete collection to Octavian, and in this form the Eclogues were perhaps published while Virgil was in Southern Italy, the error of Propertius, who describes them as written *subter pineta Galaesi*, being thus accounted for. Finally when Virgil writes, *Georg.* 4, 565,

*carmina qui lusi pastorum audaxque iuventa,  
Tityre, te patulae cecini sub tegmine fagi,*

the reference in the first line is to the original collection of purely pastoral poems which began with the words *pastorum Musam* (*Ecl.* 8, 1), and in the second line to

the present collection which began with *Tityre...* (*Ecl.* 1, 1).

The views thus summarized are set forth with great vigour, considerable lucidity and some acute criticism of particular passages. The demolition of the theory that B.C. 41 must be the date of the first Eclogue may be considered as complete. The attempt to show that the Eclogues consist of two collections is ingenious, but it is obvious that the *data* are insufficient for anything like proof. For instance, though it is easy to see in *carmina*, *Ecl.* 8, 12, 'a collection of poems,' and in *Georg.* 4, 565 an allusion to a double collection of Eclogues, yet it is equally easy to fail to see anything of the kind. The book is however one which is essential to all who are specially interested in the problem with which it deals, and ordinary students will find in it much which will help them to more fully understand and appreciate the Eclogues. Scholars, however, of the old verse-making pre-German era will read one passage with grim satisfaction. A German critic, whose commentary on the Eclogues may be had from Teubner's, is quoted on p. 92 as desirous to complete the sense of *Ecl.* 10, 36 by adding some such line as this—

*'quanta tum forem felicitate beatus.'*

T. E. PAGE.

#### MAX BONNET ON THE LATINITY OF GREGORY OF TOURS.

*Le Latin de Gregoire de Tours.* Par MAX BONNET, Chargé de Cours à la Faculté des Lettres à Montpellier. Paris, Hachette, 1890. (Pp. 1-787.) 15 fr.

M. BONNET's work, the result of eight years' labour, is a great contribution to a subject which, as he says, can only be exhausted by the combined labour of a number of specialists. A great contribution it is, such as demands the grateful and respectful recognition of scholars. I shall only attempt in these pages to give some account of its contents, and to summarize its main results.

The Introduction consists of nine sections, which treat of: (1) the subject of the work, (2) the various works of Gregory, (3) the manuscripts of the *Historia Francorum*, (4) Gregory's mother-tongue, (5) the Latin spoken in France in the sixth century A.D.,

(6) Gregory's literary education, (7) his ignorance of grammar, (8) the general ignorance of the age, (9) the principles of criticism to be applied in establishing his text. The rest of the book falls into five books, each including a number of sections: I. on Phonetics, or the changes of the Latin vowels, diphthongs and consonants during the first six centuries A.D.; II. on changes in the Latin vocabulary during the same period; III. on Morphology, *i.e.* changes in declension and conjugation, derivation and composition; IV. on Gregory's syntax; V. on his style.

The best way to give the general reader an impression of the scope and contents of M. Bonnet's book will perhaps be to summarize the Introduction, in which are set forth the results which it is the object of the succeeding discussions to substantiate in detail.



Gregory of Tours was brought up, if the expression may be allowed, among Latin surroundings. His mother-tongue was Latin, but there is nothing to prove that he did not understand Celtic. The language of the Franks he probably did not know beyond a few words. The Latin of his childhood was what is roughly called popular Latin; but what is exactly meant by this phrase? M. Bonnet's answer to this question is one of the most important things in his whole book. There is no absolute distinction at any time between popular and literary idiom. 'Personne ne songera à nommer la langue du peuple en France, d'une part, et la langue littéraire, de l'autre, deux idiomes, comme on le fait pour le latin. Si l'on veut se faire une idée de ce qu'on appellerait avec quelque raison une langue populaire, qu'on songe au patois du midi, à la langue d'oc, en présence du français. Là on a des dialectes possédant assez de caractères communs pour être considérés à juste titre comme formant une langue distincte de celle qu'on écrit et qu'on apprend à l'école et au régiment, le français. Mais dans le nord de la France, qu'appellerait on la langue populaire? Qu'entendrait on par le français populaire? Ces mots ne signifieraient rien; aussi ne les emploie-t-on pas. Ce qui existe, ce sont d'abord des patois ou dialectes; c'est en second lieu ce que nous appelons en France l'accent, c'est-à-dire une teinte de dialecte qui se fait sentir surtout dans la manière de prononcer la langue commune; c'est enfin, et particulièrement là où les patois ont cessé d'exister, une variété infinie de modifications... de cette langue commune ou langue nationale. Il est évident que tout cela ne constitue pas une langue à côté de la langue, ni une langue dans la langue. Les patois ont tous, avec la langue régnante, des rapports assez étroits pour se reconnaître en elle sans peine, et ils représentent, non pas une seconde unité, mais la diversité, la pluralité en face de l'unité. Les influences du dialecte local sur la langue commune sont aussi nombreuses que les dialectes eux-mêmes, et infiniment variables en intensité. Enfin, prétendra-t-on qu'on puisse opposer au bon français, sous le nom de français populaire, un mélange dans lequel entreraient les parisianismes ou les provincialismes de la classe bourgeoise; les fantaisies de l'argot des collégiens, des étudiants, des militaires, des comédiens, une quantité, si grande soit-elle, de fautes de prononciation,... enfin, ces expressions et ces tournures assez nombreuses dont on se sert sans scrupule en parlant et qu'on évite en

écrivant? C'est pourtant tout cela, tout ce qui, à Rome, correspondait à cela, qu'on prétend enfermer dans cette dénomination de latin populaire; c'est à cela qu'on prête les caractères d'un véritable idiome.'

The Italians who came to France brought their Latin with them, each clan probably its own Latin, merchants, officials, legionaries, agriculturists, professors. 'Les négociants avaient leur vocabulaire, les artisans le leur, les agriculteurs de même; chacun avait aussi une prononciation et des formes de langage différentes selon le niveau de son éducation.' In the same way the French who learned Latin must have differed in rank, in occupation, and consequently in language. 'Le partage d'une nation en lettrés et en illettrés, en savants et en ignorants, est une fiction; nul n'est impeccable, et personne n'ignore l'existence d'une règle.'

It is easy also to exaggerate the degree of fixity which attaches to a written language. The literary idiom, like the spoken idiom, which it always to a certain extent represents, is liable to change. On the other hand, the spoken language has never been independent of the written; the laws of the latter are not entirely without their effect. The pronunciation of Latin, it is true, went on changing; but here again it would be wrong to draw a hard and fast line between the Latin of the educated and that of the uneducated. 'La prononciation qu'on appelle vulgaire était sans doute à peu de chose celle de tout le monde. Ce qui était vulgaire, c'était de laisser percer cette prononciation dans son orthographe.' The same considerations apply to the degeneration of Latin declension, conjugation, and syntax, as well as to the enlargement and alteration of the Latin vocabulary.

The Latin of Gregory's written works, though falling far short of classical purity, is less affected by barbarisms than other writings of the same epoch. It reflects a conflict between the natural incorrectness of his everyday conversation, and the ambition of the imperfectly educated man. For (as he himself assures us) he was ignorant of grammar, and, though conversant with the Bible in a pre-Vulgate version, and with much Christian literature, his classical reading seems hardly to have gone beyond some Vergil and a little Sallust. In all this he is only a child of his age: an age in which the tradition of liberal culture had almost died out.

While the manuscripts of Gregory's minor works (to the previous accounts of which M. Bonnet adds something) are comparatively



late, and leave the text in consequence somewhat uncertain, those of the *Historia Francorum* represent, in large part, a copy written as early as the seventh century, and therefore nearly contemporaneous with the historian himself. The untutored Latin of Gregory was often corrected by the copyists of the Carolingian era, but a modern editor will endeavour where possible to restore the truer, though less grammatical, text.

Such are the general theories upon which M. Bonnet bases his great study of Gregory's Latinity. It would be impossible within these limits to follow him through the details of his analysis. But it may perhaps be worth while to remark that the incorrect uses of prepositions with cases (e.g. *cum* with the accusative), though it had begun very early, as the Pompeian inscriptions testify, is not one of Gregory's weaknesses. The form *eclesia* for *ecclesia* (Bonnet p. 157), which he often uses, is found on an inscrip-

tion of Vienne of the date A.D. 557 (*C.I.L.* 12, 2085) and on one of Aix (*C.I.L.* 12, 5787); in Africa too it is not uncommon. Other omissions of consonants (as e.g. in *operiens, oportunus*) may be paralleled by *ofensa* (Narbo, *C.I.L.* 12, 4975) and *oficio* (Vienne, *ib.* 2169). The substitution of the fourth for the third conjugation in *innectire, suggerire* etc. (Bonnet p. 431) may be illustrated by *gemire* in a French inscription of the end of the fifth century (Orange, *C.I.L.* 12, 1272) and another at Vienne of the year 579 (*ib.* 2094). The confusion between *quem* and *quod* (p. 509) may be illustrated from an Aix inscription of the third century (*C.I.L.* 12, 2461). But the accusative absolute, to which Gregory and other writers of his epoch have no objection (p. 561), is not found in any of the French inscriptions edited in the twelfth volume of the *Corpus*. It appears, however, in Africa as early as the age of Diocletian (*C.I.L.* 8, 4551, 8924).

H. NETTLESHIP.

#### TAYLOR'S WITNESS OF HERMAS TO THE FOUR GOSPELS.

*The Witness of Hermas to the Four Gospels.*

By C. TAYLOR, D.D., Master of St John's College, Cambridge. London: C. J. Clay & Sons, Ave Maria Lane. 1892. 4to, pp. viii. 148. 7s. 6d. net.

It is generally thought that the *Shepherd* of Hermas is of little or no value for the history of the Canon. The nature of the work indeed does not lead us to expect any very distinct references, much less quotations. It is, says Bp. Lightfoot, 'equally devoid of citations from the Old Testament and the New.' Nevertheless Dr. Taylor has been led to think that its testimony to the Gospels is strong and convincing, and that it says in effect that the number of the Gospels was actually and necessarily four, as Irenaeus said after it. Dr. Taylor was led to his inquiry by his examination of the *Shepherd* undertaken for a different purpose, namely to ascertain the relation between it and the *Didaché* and to decide which borrowed from the other. The result of that examination was that Hermas not only used but used up the *Teaching*, so that anything very striking in the latter was sure to be found in some disguise or other in the former. A point of great importance resulted from this investigation, namely 'the discovery of

his [Hermas'] way of using his authorities. He allegorizes, he disintegrates, he amalgamates, he plays upon the sense or varies the form of a saying, he repeats the words in fresh combinations or replaces them by synonyms, but he will not cite a passage simply and in its entirety.' Now finding in the *Teaching* reference to 'the Gospel of our Lord' Dr. Taylor naturally asked: Is there any disguised trace of the word 'Gospel' in Hermas? The answer was found in the words ἀγγελία ἀγαθή in *Vis.* iii. 13, 2. The words are found in connexion with the third vision of the lady who represents the Church. In the first vision she appears as an aged woman representing the Church under the old dispensation. In the third she is young and joyous. The reason of this is given in the interpretation. When to one in sorrow there comes good tidings, ἀγγελία ἀγαθή, he forgetteth his former sorrow. Now as the thing represented is the Church under the Gospel, it seems clear that these words are used as a synonym for εὐαγγέλιον. Then follows the statement: 'Whereas thou sawest her seated on a bench (συμβέλλον, = *subsellium*) the position is a firm one, for the bench has four feet and stands firmly; for the world likewise is compacted of four elements.' The reasoning seems so inept that it would

be flattery to call it puerile. How could it enter any one's head to give as a reason for the firmness of a four-footed bench the fact that the world is constituted of four elements? But if we suppose that what Hermas has in his mind is the fourfold Gospel the passage becomes intelligible. Irenaeus also argued that the Gospels cannot be more or less than four because there are four regions of the world and four catholic winds.

It may be objected that the four feet merely indicate firmness; but this would be to miss the point of Dr. Taylor's argument, which is not based on the simple mention of the number four, but on the reason added. The strength of his argument lies in the utter incoherence of Hermas' reasoning on any other view. The reasoning implies that the number four was something accepted as a fact in the system of things. This is certainly strongly confirmed by the fact that Irenaeus uses a precisely similar comparison. The manner in which the number is introduced agrees with this. In the vision itself nothing was said of the four feet, only the lady was seated on a bench; it is in the interpretation that the number is mentioned. In the ninth *Similitude* again we have a tower, the spiritual counterpart of the Creation, its foundation consisting of four tiers which in the interpretation are explained to mean the four ages of the world. So Irenaeus states that the Word revealed himself to all the four generations, and each of them received a covenant, each revelation and covenant corresponding to one of the Canonical Gospels. The last generation receives the actual Gospel, which comprises the Four Gospels. 'The Church in Irenaeus has the Gospel for its one pillar and the Gospels for its four pillars: analogous to this in Hermas are the figures of the one bench with four feet, and the one foundation with its four rows or tiers representing the Gospel and the Gospels.' Irenaeus, we learn from

Eusebius, not only knew the *Shepherd*, but received it as Scripture. We may not unreasonably conclude that he adopted these figures from that work.

Dr. Taylor proceeds to search the *Shepherd* for traces of Gospel sayings. The principal interest of this part of the inquiry is of course in connexion with the Fourth Gospel. In consequence of the peculiar way in which Hermas deals with his authorities, it must be obvious that no fair idea of Dr. Taylor's investigation could be given by quoting two or three of his parallels. It is necessary that the reader should first learn from Hermas' use of the *Didaché* and the Synoptics in what form we may expect to find his allusions. Nevertheless a striking one may be mentioned. Hermas asks (*Simil.* ix.) the explanation of the rock on which the tower is built and the gate (*πύλη*). He is told that they are the Son of God, and the gate is made new that those who are to be saved may enter by it. The stones for the tower are also brought in through the gate by the Virgins, but some stones are found which were not brought in by them nor through the gate, and these are unsuitable in the building and are carried away to the place from which they came. The correspondence of all this with the words in the Fourth Gospel 'I am the door, by me if any man enter in, he shall be saved' is very close. The change of *θύρα* to *πύλη* is exactly in accordance with Hermas' manner.

The date of Hermas according to the author of the Muratorian fragment is about A.D. 140—150, during the episcopate of Pius; but there are good authorities who accept the claim he makes himself to be contemporary with Clement. In any case, if Dr. Taylor's argument is sound, the Four Gospels must have attained their canonical and exclusive position a third of a century before the statement of Irenaeus that the Gospels must be four and four only.

T. K. ABBOTT.

#### BATIFFOL ON THE SOUTH-ITALIAN MSS. IN THE VATICAN LIBRARY.

*L'Abbaye de Rossano, contribution à l'Histoire de la Vaticane*, par PIERRE BATIFFOL. Paris: Picard. 1891. 7fr. 50.

IN this 'contribution to the history of the Vatican' M. l'Abbé Batiffol continues the researches he began in his *La Vaticane de*

*Paul III. à Paul V.*, published in 1890. In that volume he took as his basis the manuscripts from Grotta Ferrata that have entered into the Vatican Library, and illustrated from them points in the history of that monastery, and in the lives of the Cardinals Sirleto and Carafa. In his new

volume of 182 pages, Sirleto and Grotta Ferrata appear again, but as parts of a wider question. M. Batiffol deals with no smaller a subject than the history of Greek culture in the South of Italy, as contained in the foundation, life and extinction of the Basilian monasteries of Magna Graecia and Sicily, and especially of one of them, S. Maria del Patire at Rossano.

M. Batiffol's book is a specimen of a class of which France has lately given us several striking examples. A number of manuscripts are taken, now part of a great collection, but which came from the library of some other institution. From technical indications the number of these manuscripts is ascertained, an account is given of the history of the institutions to which they formerly belonged, of their still earlier owners, of the epoch at which they were written, and of the culture of that period. M. Batiffol starts from sixty-one manuscripts now among the 'Vaticani graeci'; these are a part of a smaller collection known as the 'Basiliani,' transferred to the Vatican from the house of St. Basil in Rome. But they were by no means originally there. The whole collection was one made at the beginning of the eighteenth century by sweeping in MSS. from Basilian monasteries in South Italy, and in this general gathering was included S. Maria del Patire at Rossano. But many of the MSS. are older than the foundation of that monastery. Only their handwriting can tell us where they were written. M. Batiffol is therefore led to discuss this question, and that of the extent of the Hellenisation of the South of Italy. Thus, the history and geography of South Italy, the foundation and fortunes of one principal monastic establishment and its various offshoots; the course and decay of the Basilian order, attempts at its reform, relations with historical personages and with the Court of Rome, the removal of the libraries and the suppression of the order, arise naturally from the identification of some sixty Vatican manuscripts.

The contents of the book are arranged in the following order. Introduction, *La Grande Grèce Byzantine* pp. i.—xl. Geography and history of South Italy with regard to the Greek element among its inhabitants. An important passage is one in which M. Batiffol states his view of the origin of the Italiote Greeks (p. v.). He denies that South Italy was peopled by the Byzantine monks who in the eighth century fled from the persecution of Leo the Isaurian; he ascribes to two events the

Grecising of Italy, namely the gradual migration of Greeks from Sicily from the latter half of the seventh century onwards, a result of the expedition of Constans II.; and secondly, and chiefly, the taking of Sicily by the Saracens in the ninth century. M. Batiffol therefore derives the Greek culture of Italy from the West and not from the East. Chapter I. *L'Abbaye du Patir*. Foundation of S. Maria del Patire (that is del Padre, from the founder) by St. Bartholomew in the year 1105. Foundation from it of S. Salvatore at Messina, and S. Elia de Carbone in the Basilicata. Chapter II. *La Librairie du Patir*. A list of the MSS. forming the library, so far as they can be recovered from explicit entries in the books themselves, press-marks, similarities of hand and other indications. The collection of the manuscripts of S. Maria del Patire and of the Basilian monasteries generally, made by Pietro Menniti the head of the order, about the end of the seventeenth century; their transfer to the Vatican under Pius VI. about the year 1780.

The historical part of M. Batiffol's book is ill represented by the analysis I have given, but a serious criticism of it would be beyond the power of the present writer, and it will doubtless meet with full recognition elsewhere. I must content myself with expressing the admiration with which I have read M. Batiffol's patient and at the same time interesting exposition. He has increased the debt that students of the Vatican and of Italy already owe to Frenchmen.

M. Batiffol's third chapter *Origines de la Librairie du Patir* calls for a more detailed notice. The MSS. of S. Maria del Patire are partly signed, partly unsigned; of those that have signatures some come from the East, some were written in Italy. It was necessary to assign a source to the remainder. In so doing, that is in claiming this or that origin for them on the evidence of their handwriting, M. Batiffol is led to lay down general distinctions between the Eastern and the Western Greek hand. This question, like all questions of historical palaeography, has a great interest and importance. Hitherto the advance made towards its solution was represented by the attempts of Prof. Gardthausen, of which a recapitulation (pp. 85, 86) is given by M. Batiffol. These investigations had not given any very decisive result. M. Batiffol on the other hand presents us with detailed and definite criteria for identifying

Italian Greek MSS. of the tenth century and onwards. He remarks justly (p. 86) that Gardthausen looked too exclusively for characteristics of *writing*, whereas handwriting in itself is comparatively vague and liable to rapid degeneration and assimilation to different types. Decoration, on the other hand, is obvious and defined, offering more elements to observation and more easily recognised in its decay. M. Batiffol accordingly describes Italian Greek MSS. in these two relations, hand and illumination. As to handwriting and general arrangement he says (p. 89): 'le parchemin est mal poli, mal blanchi, mal réglé; <sup>1</sup> l'encre brune et pâteuse. L'écriture est inégale, droite, drue ou (si l'on veut) très tassée; ce caractère est plus sensible à qui compare l'écriture égale souple et aérée des copistes Byzantins.'

As to the decoration, (which as in Eastern MSS. extends to titles, initials and bands or head-lines at the beginning of books or chapters), while in the Byzantine style the outline of the figure or letter is drawn in red, gilded, and colour is then added inside, in Italy 'toute cette décoration est tracée à l'encre, à la même encre que le texte, puis les pleins du tracé sont coloriés, mais sans or et sans aucune gouache, de vert, de violet, de rouge, de jaune, de bleu, toutes couleurs posées à teintes plates... Les initiales sont généralement *animées*'; they represent hands, heads, birds, serpents and the like. 'Les initiales moindres sont de petit onciale de même encre que le texte, mais on les a barbouillées d'un coup de pinceau, en jaune, souvent aussi en vert et en violet.' The smaller titles also have this characteristic mark that they are drawn with the same pen as the text, and decoration is imparted to them by a simple dash of colour. These characteristics—a vigorous, somewhat inelegant, close-packed hand, yellow parchment, careless arrangement of lines and rulings, together with a simple, rough, and grotesque illumination—are naturally most distinct in the century when the school begins, the tenth. We find them in MSS. written between A.D. 959 (the date of Vat. 2027) and the first half of the eleventh century. M. Batiffol traces their history through the following centuries (p.

92 sq.). The hand, in the general decay of Greek writing, assimilates itself rapidly to the current Eastern type, and the decoration, also reduced and starved, remains the only means of detecting the Italian origin of a MS. The characteristics of this Italian-Greek school at its beginning have much in common with Lombardic Latin writing; and the MSS. that display the characteristics in the most marked manner are written not far from Beneventum—such as the Vaticani 2138 and 2020, written in A.D. 991 and 993 in a monastery near Capua, and two MSS. at Monte Cassino (nos. G 277 and 278). Geographically therefore as well as palaeographically M. Batiffol finds a relation between Western Greek writing and Lombardic Latin, and he gives the school the name of 'gréco-lombarde' (p. 91).

It is unnecessary to point out the importance of this conclusion, widely based and clearly expounded. The present reviewer can claim acquaintance with most of the documents treated by M. Batiffol; and, writing from the *ἀγιον ὄρος* of St. Benedict, thronged with peasants at Pentecost, he cordially recognises the accuracy of his observation and the sureness of his combination.

Some general remarks may be added. For classical philology the results of this book are not of great moment. The 'culture' of the Italiote Greeks was almost entirely ecclesiastical. A few profane MSS. may, as the Renaissance approached, have been written at Messina or at St. Niccolò di Casola, but we look in vain among the books written by order of St. Nilo or St. Bartholomew for a copy of a pagan author. M. Batiffol's criterion of illumination also is of less value when applied to classical books, which usually possess the minimum of colour. It is true that his standard marks the Italo-Lombardic hand as one to which we need not look as the source of our classical manuscripts; but we need criteria within the Eastern world itself, and other rules to tell us the distinction between the hand of Constantinople and that of the Morea; we need to know whether there were independent centres of Greek writing, or if, as M. Batiffol seems to think, the capital Constantinople gave the lead and the provinces copied, with more or less success, the style of the metropolis. We know that the Clarke Plato was written at Patras, about a century before the typical specimens of the Greco-Lombard school; where was the Paris Plato (grec. 1807) written? And the crowd of classical MSS. that date from the end of the

<sup>1</sup> I may add, on this subject, that while Eastern MSS. are rarely if ever ruled otherwise than on the hair-side, in Italy less distinction seems made between one side of the parchment and the other. In the MSS. Monte Cassino 278, Vaticani 2020 and 2138, Grotta Ferrata B. a. i, B. a. iv., Δ. γ. i. the rulings are regularly on the flesh-side; in Vaticani 1553, 1636, 1666, 1808, 1809, 2056, 2094, Messina 86 and 116, the rulings are alternately on either.



tenth century—the Venetian Iliad, the Ravenna Aristophanes, the Anthology, manuscripts of Demosthenes, of Aristotle, of the Tragedians, are these Constantinopolitan or provincial?

There is one characteristic of the Italo-Greek scribes that M. Batiffol does not notice, namely the abbreviations that they so frequently use. This, if not a perpetual property, is a very frequent accident, and when it occurs is almost as decisive a mark of the school as the writing or the illumination. Thus of M. Batiffol's typical MSS. the Cryptenses B. a, iv., B. a, iii. and others of Grotta Ferrata, the Vaticani 1633, 1658, 1673, 2067, and several others, are highly tachygraphic. And the recognised sources of tachygraphy, Vat. 1809, the British Museum MS. Add. 18234, the MS. Angelica B. 3. 11., have all been at Grotta Ferrata and were written in the South of Italy: the MS. Vat. 1982 came from the monastery of St. Elias de Carbone, of which M. Batiffol gives us the history, the Tropologium Vat. 2008 from St. John Theristes at Stilo. To these I can add upon the strength of M. Batiffol's canon a MS. of which Signor Vitelli has published the tachygraphy (*Museo Italiano* I. p. 9 sq.), but the Lombardic origin of which had not been suspected, Laur. Conv. Soppr. 177 (from the Badia di Fiesole). In the same way the unusual abbreviations of Vat. 1611 (s. xii.) incline me to regard it as Western, unless the mention of the *σχολή τοῦ ἁγίου πέτρου*, for which it was written, is decisive for a Byzantine origin (p. 83).

At the end of his book (pp. 103, 104) M. Batiffol makes a little group of four MSS. which offer somewhat different charac-

teristics—Vaticani, 1456, 2000 (but in this MS. only four pages, ff. 30–33, come into question), 2061, and 2066 (uncial). By an oversight Vat. 2067 is omitted, the first 200 pages of which are certainly in this hand, (s. x.–xi.), while the remainder of the book and all the marginalia are in a later and different hand. Parts of Vat. 1974 (ff. 71–102, 121–125) also belong to this hand. The characteristic of this school is that of a very linked and cursive minuscule, with peculiarities in the letters iota and kappa, and a marked uncial nu. M. Batiffol, finding an Arabic palimpsest in some of the leaves of Vat. 1456, is inclined to localise the hand in Calabria. The question, as he admits, requires more evidence; the hand certainly occurs more often than in these examples.<sup>1</sup> I should prefer to say nothing about locality, but the strongly cursive character of the hand suggests to me a connection with the very remarkable cursive-minuscule MS. Vat 2200 (s. viii.–ix.), a page of which has lately been facsimiled by the Palaeographical Society.

P. 151. The word in the subscription of Vat. 1611 which is printed *μοῦλτ* is *μοῦλτον*. I do not offer any suggestion as to its meaning. P. 156. In the subscription to Reginensis 75 M. Batiffol makes a lacuna after the word *μαλβρο*; Sig. Stevenson in the catalogue prints what stands in the MS., namely the ordinary symbol for *εἰς τὸ*, which should be read. Is it correct to say (p. 104), that palimpsests were unheard of at Constantinople?

T. W. ALLEN.

<sup>1</sup> E.g. in the MSS. Messina 116, Vaticani 2084, 2089, 2115.

#### WRIGHT ON THE DATE OF CYLON.

*The Date of Cylon*, by JOHN HENRY WRIGHT. Reprinted from the 'Harvard Studies in Classical Philology.' Boston: U.S.A. Ginn and Co. 1892. (80 pp.)

'This paper was originally prepared in 1888 and was read before the American Philological Association at the meeting of that year; in the summer of 1890 it was re-written for publication in the *Harvard Studies*. Since that time the publication of the *Ἀθηναίων πολιτεία* has completely confirmed the correctness of the writer's chief

contention—a pre-Draconian date for Cylon. The paper has accordingly been revised and in part rewritten.' This extract from the introductory note gives the history of the pamphlet: it is doubtful whether, since the ostensible object of the author is to prove that the attempt of Cylon belonged to the period before Draco, and since this may now be considered certain, it was wise to republish the work in its present form. The author however deserves credit for having followed Busolt in a view which has now been confirmed; and he incorporates in

his work a full discussion of many of the difficulties of early Athenian history as well as a valuable review of the authorities and their relations to each other. He thinks that Plutarch did not have the unabridged *Respub. Ath.* before him: 'the resemblances, the dissimilarities, and the discrepancies alike are intelligible only on the supposition that Plutarch was transcribing from some work in which an abridgment of these parts of the *Respub. Ath.* was embodied.' The most important part historically is a short account of the history of the Alcmaeonidae before Peisistratus, which contains useful chronological work. A suggestion in a note (p. 43) that the word *εὐπατρίδαι*, at least before the time of Aristotle, was not used in the technical sense to which we are accustomed, deserves special attention. The rarity of it in prose writing is certainly remarkable. *Xen. Oec.* 1. 17 (to which he does not refer) is doubtful. The best instance of its earlier use is Euripides *Ion* 1069 etc.

οὐ γὰρ δόμων γ' ἑτέρους  
ἀρχοντας ἀλλοδαποὺς  
ζῶντά ποτ' ὀμμάτων ἐν φαεναῖς ἀνέχουσ' ἂν  
αὐγαῖς  
ἁ τῶν εὐπατρίδων γεγῶσ' οἴκων

which also seems to have escaped his notice. This with the Scolion which he quotes from *Ar. πολ. Ἀθ.* 19 seems decisive for the old-fashioned view. Also if the statement in the *Ἀθ. πολ.* ch. 13 is correct, that five archons were to be selected from the *εὐπατρίδαι*, the word must have had its technical meaning from the earliest times.

The work throughout shows learning and diligence; the author is thoroughly versed in the ancient and modern literature; there is a want of sense of proportion in the devotion of 80 pages to an argument which could have been clearly stated in a quarter of the space; the argument would however have been almost conclusive even had no further support been forthcoming. The attempt to fill up the bare outlines of the history and to show that the episode of Cylon is not a detached incident in Attic history, but 'reveals itself as one of the most interesting and significant steps in the social and political development of pre-Solonian Athens,' while it is closely connected with the establishment of the date, is a good piece of historical writing and is a very satisfactory *résumé* of what can be made out from the very scanty evidence.

J. W. HEADLAM.

#### FALKENER'S ANCIENT GAMES.

*Games Ancient and Oriental, and How to Play them.* By EDWARD FALKENER. Longmans: 1892. 21s.

THE contents of this volume are further described in the title-page as 'the games of the ancient Egyptians, the Hiera Gramme of the Greeks, the Ludus Latrunculorum of the Romans, and the Oriental games of chess, draughts, backgammon and magic squares.' Only a few of these games, it will be seen, come within the scope of this *Review*; those, namely, which have been identified, or sought to be identified, as practised by the Greeks and Romans. Egyptian tombs have yielded a large number of pictorial representations of different games, and some smaller remains of the actual boards and men with which they were played. The classical writers, on the other hand, have left a variety of descriptions and incidental allusions more or less intelligible, but never quite sufficient to give a clear

notion of the games to which they referred. It was a happy thought to combine these two sources of information, and further to bring into the comparison games actually played in the East in modern times. This plan has been carried out with great ingenuity, and the author's travels in Egypt, Asia Minor, and as far as China and Japan, extending it would seem over a long series of years, have been brought to bear on the various questions of identification. He claims to have solved, by this comparative method, difficulties which had baffled all previous inquirers.

'As the Egyptian game of *Tau*, or Robbers, and the Roman game of the *Latrones* or *Latrunculi*, or Thieves, were incapable of solution when considered separately, and resisted all attempts of the learned to explain them; though each has explained the other when the references to the Roman game were applied to the board of the Egyptian game: so the Greek and Roman games we are now about to consider have remained up to the present time mere abstract ideas, known only by name; while the

Egyptian games, when seen in our Museum, was known only by form. But no sooner do we compare the two together, then we find them one and the same thing; and are thus enabled to make each intelligible; and thus, as in the games of Tau and the Latrunculi, in finding out one game we discover two' (pp. 91—2).

We readily admit that the close resemblance, perhaps the identity, of several ancient and modern Egyptian games has been established. When we come to the Graeco-Roman games, however, the proofs break down at critical points, owing to the almost entire neglect of the Greek authorities, and the very partial use made of the Latin ones.

Nothing, for instance, is better attested than that the Duodecim Scripta of the Romans was a game to all intents and purposes the same as our backgammon. The researches of Becq de Fouquières and Dr. H. Jackson, followed in the *Dictionary of Greek and Roman Antiquities*, have clearly shown that the division of the board into lines, the number of men, the way the throws counted, the hitting of 'blots' (ἀζύγες), the final clearing off of the men, were identical in the two games. Mr. Falkener writes with Becq de Fouquières before him, but does not mention the epigram of Agathias (*Anth. Pal.* ix. 482 = Brunck *Anal.* iii. 60); and Duodecim Scripta without Agathias is indeed the play of Hamlet with the Prince of Denmark left out. By an excessively strained use of some other passages, he proceeds to prove that the Duodecim Scripta are to be identified with one of the forms of latrunculi, or draughts, which he calls (in his title-page) 'the Hiera Gramme of the Greeks.' In reality our texts give no hint that ἱερὰ γραμμὴ was the designation of a game; they tell us that there was a game called πέντε γραμμαί or the five lines; that each player had five men (ψήφοι) moving upon five lines; that between the two was a line called ἱερὰ γραμμὴ or the sacred line; that in some unexplained way to move the man from this central line was dangerous, so that κινεῖν τὸν ἀπ' ἱερᾶς became a proverbial expression for 'to try one's last chance' (Pollux ix. 97: quoted in full, in the Greek, in *Dict. Antiq.* s.v. *Latrunculi*). Mr. F. does not know that γραμμὴ can only mean a line; he finds an Egyptian board of twelve squares by three; the middle row of squares becomes the ἱερὰ γραμμὴ, or, as he translates it 'The Game of the Sacred Way'—a reminiscence rather of Attic or Roman topography than of any game to be read of in ancient authors. The rest is easy:

'If we accept these boards as representing, and being identical with, the Greek and Roman games, then all doubt is at an end; for the 'duodecim' of the latter is represented by the twelve squares of the former' (p. 95).

Other people have suggested that Duodecim Scripta must be twelve lines as in backgammon: 'but the word scribo may be equally understood to draw spaces or squares as to draw lines' (p. 98). 'The central column,' he explains, 'was common to both players. This was the Sacred Way, on entering which [he does not say how] each party would strive to take up the other's pieces, and arrive at the goal.' The following extract speaks for itself:

'The unexpected way in which the pieces are often taken up or removed from the Sacred Way accompanied by the expression κινεῖν τὸν ἀπ' ἱερᾶς, I remove this from the Sacred (Way), passed into a proverb; just as we should speak of any one being 'removed from the stage of life' (p. 99).

No attempt has been made, it is clear, to ascertain the true meaning and application of the proverb. And we have the further confusion between a game like backgammon, of mixed chance and skill and played with dice, and a game like draughts, of skill only. The game called πέντε γραμμαί must, in fact, have been a very simple form of draughts, played on lines not on squares. When Sophocles says καὶ πεσσὰ πεντέγραμμα καὶ κύβων βολαί, and when Plato (*Phaedr.* 274 D) ascribes the invention of περτεία and κυβεία to Theuth, the Egyptian Hermes, this does not prove that the game of πεσσοί was played with dice; two distinct games are referred to.

Another more developed form of draughts was played with thirty men on each side, and on a board divided into squares. This is identified by Mr. F. with the Egyptian Tau, or game of Robbers, and (less convincingly) with the Roman Ludus Latrunculorum. The number of squares is not mentioned by any ancient writer; Mr. F. with much probability fixes it at 144 from Egyptian sources, and suggests that the thirty men were arranged in five rows of six each, on alternate squares as in the modern game of draughts. There would thus be only two vacant rows, and the opposing forces would soon get to close quarters. On several other points we are constrained to differ. The name διαγραμμασμός belongs apparently, not to this or any other form of draughts, but to Duodecim Scripta or backgammon. Again on the evidence of passages quoted at length in the *Dict. of Antiq.* we hold that πάλαι was not the name of a part of the board, or a group of pieces, as

Becq de Fouquières and Mr. F. maintain, but that the squares themselves were called anciently πόλεις, and later χώραι. The *Dictionary* likewise gives in full the proofs that hemming in the enemy was of the essence of the game. Of all these passages a portion of one only is quoted by Mr. F., and that, significantly enough, in the Latin version (ἡ τέχνη τῆς παιδιᾶς ἐστὶ περιλήψει δύο ψήφων ὁμοχρόων τὴν ἑτερόχροον ἀνελεῖν, *Pollux* ix. 98). The Latin passages bearing on *Latrunculi*, collected originally by Salmasius and Hyde, are set out at sufficient length; but in adapting them to Mr. F.'s theories some very forced interpretations occur. Thus we read in Isidore's *Orig.* xviii. 67: 'Calculi partim ordine moventur, partim vage. Ideo alios ordinarios, alios vagos appellat. At vero, qui omnino moveri non possunt, incitos dicunt.' The plain meaning of this is, that there were at least two kinds of pieces, some moving with greater freedom than others; if not also a third class, the *inciti* or immovable, as Mr. Tilley suggests (*Class. Rev.* VI. 335). Mr. F. decides, on subjective grounds, that the pieces must have moved all alike: so much the worse for the facts.

'I apprehend therefore,' he says, 'that the passage merely means that all the pieces move both in an ordinal or straight line, forwards, sideways, and backwards, and in a diagonal line; and that those that "cannot move" are called by such a name, and are then taken off' (p. 50).

This is subsequently explained to mean that

'All the pieces, though moving only one square at any time in any direction, could leap over an adversary occupying a contiguous square, provided the next square were open, as in draughts, but without taking it.'

After this we need not follow Mr. F. in

his examination of the passage from the *Panegyric on Piso*. To do him justice, he is right in the main as to the sense of *alligare*, meaning to attack. A piece which was *alligatus* was not necessarily lost; it might escape as long as it was attacked by only one enemy, but was taken off the board when caught between two. Like most writers on the subject, he has failed to distinguish between *ligatus* and *similis ligato* (*Paneg. Pis.* 189). The identity of the Greek πόλεις and the Roman *latrunculi* is not yet proved; where they differed was probably, as Mr. Tilley has well pointed out, in the more military character of the Roman game.

We have considered Mr. F.'s book exclusively from the classical point of view, and have had occasion to criticise his mode of dealing with Greek and Latin authors. In other respects we have found his work full of interesting matter. He professes to teach us 'how to play' these games; and this is a point we must confess not to have put to a practical test. Much of the information about Egyptian games is not to be found elsewhere. Under this heading the author's obligations are freely acknowledged to the late Dr. S. Birch and to Mr. P. le Page Renouf, of the British Museum. Mr. Renouf (*quem honoris causa nomino*), in a letter otherwise filled with Egyptian and Oriental learning, ventures on the following remarkable statement: 'It is, I believe, quite true that the original word for pawn was *paon* (peacock).' In Sanscrit, where the word first appears, as elsewhere, 'pawn' has always meant simply 'foot-soldier': the Italian *pedone* gives the typical form I of which Sp. *peon* and Fr. *pion* are contractions.

W. WAYTE.

**Livy, Book I., and Livy, Book II.** With Notes by J. PRENDEVILLE. Re-edited and partly rewritten from a revised text by J. H. FREESE, M.A. Cambridge: Deighton Bell & Co. London: George Bell & Sons: and New York, 1892. 1s. 6d. each.

IN these two little volumes 'the text, with slight variations, is that of Weissenborn (Teubner 1889).' Each contains about twelve pages of useful and clear introduction, of which the first seven or eight dealing with the life of Livy and sources of early Roman history are identical. Each also has two maps, one of Rome and one of its environs. The notes are clear and to the point, and will be found to give the necessary assistance to careful readers. They are not intended for the lower forms of a public school.

In the Introduction it is stated that according to Tacitus in *Ann.* iv. 34 'Augustus nicknamed' Livy 'Pompeianus.' All that Tacitus there puts into the

mouth of Crematius is the statement that 'T. Livius.....Cn. Pompeium tantis laudibus tulit ut Pompeianum eum Augustus appellaret,' 'called him a Pompeian'—which is quite a different thing.

On p. xix. of the Introduction to Book ii. it is said that the *comitia curiata* 'was exclusively patrician.' Such, it is true, was its original constitution, but subsequently (perhaps on the expulsion of the Kings) the plebs was enrolled in the *curies* and was therefore capable of voting in the *comitia curiata*.

On p. xx. of the same Introduction it is said that the tribunes and aediles of the plebs were probably elected by the *comitia tributa*. It is added that 'the plebeians alone had the right of voting in this assembly.' This is probably inaccurate. The plebeian magistrates were elected by a *concilium plebis tributum*, and not by the *comitia tributa* which was an assembly of the whole *populus*, patricians as well as plebeians.



In the note on i. 14, 3 it is said that *inde non modo commune sed concors etiam regnum duobus regibus fuit* (i. 13, 8) implies that 'there existed a feeling of latent distrust.' But how can it imply this?

In the note on i. 21, 5, "*Argyos pontifices* : 'the Argive chapels' : " *pontifices* which has got in by a slip of the pen should be erased. The word is of course the nominative to *vocant*.

In the note on i. 26, 7 (*hac lege duumviri creati*), *qui se absolvere non rebantur (ea lege) ne innoxium quidem posse, cum condemnassent* is translated 'who imagined they could not acquit even a guiltless person after they had found him guilty.' In the subsequent comment the editor explains this so as to give it some meaning; but he has not noticed that he has left *duumviri* without a verb. He should have translated : 'When the judges, who thought that according to that law they could not acquit the man even if innocent, had passed sentence on him.'

In ii. 30, 4, *suo vehemens* (if it means anything at all) cannot mean 'which was in its own nature uncontrollable.' The text and notes have not been brought into harmony here.

On p. 48 of the same book a full stop has got into the middle of a sentence in the fifth line from the bottom.

*Nec in praesens modo* ii. 42, 7, requires, I think, a note to show that it is to be taken with *vicit*.

M. T. TATHAM.

**Thirteen Satires of Juvenal.** Translated into English by ALEXANDER LEEPER, M.A., LL.D., Warden of Trinity College in the University of Melbourne. New and revised edition. Macmillan, 1892.

DR. LEEPER informs the reader that this translation is a new version of the rendering originally brought out by him in conjunction with Dr. H. A. Strong, now Professor of Latin in University College, Liverpool. Dr. Leeper's translation is interesting not only as being accurate and spirited, but also as being honestly written in the English of the last two decades. It strikes one occasionally as needlessly harsh and crude, sometimes because the author is over anxious to be literal, sometimes because he is too careful to employ short English words; e.g. (p. 116) " 'Let go the cable,' cries the owner of the corn or pepper that has been bought up." The celebrated passage in the seventh satire *Satur est cum dicit Horatius Euhoe* etc. is thus given (p. 41) : "He has dined has Horace when he shouts his 'Evoe.' Your heart admits not of divided interests—what room is there for a poet's frenzy, unless the only restlessness they feel is that of inspiration, unless they career along in the train of the Lords of Cirra and Nysa? It was the creation of a great mind (agitated, if you will, but not about the price of a blanket) that vision of chariots, and horses, and faces celestial, and of the Fury's form when dazing the Rutulian. Were Vergil left without a slave and decent lodging, then every snake would tumble from his locks: his trumpet would be hushed, and sound forth no impressive notes." 'Agitated' is not strong enough for *attonitue*, nor is 'dazing' an adequate equivalent for *confundat*, nor 'impressive' for *grave*. And surely 'his locks,' 'his trumpet' should be 'her locks,' 'her trumpet.'

The following may be cited as a favourable specimen of Dr. Leeper's general style:

P. 11. *Quid Romae faciam?* 'What am I to do at Rome? I do not know how to lie. When a book is bad I cannot praise it and beg the loan of it. I am a dunce at astrology, and as for guaranteeing a parent's demise, I neither will nor can. I have never inves-

tigated the entrails of frogs: conveying to a married lady an admirer's presents or messages—others know how to do (? this). No rogue shall ever have my help; and so I go out in no governor's suite, just as though I were a cripple—a useless lump with a withered hand. Who has a friend now, unless he be an accomplice, unless his fevered heart throb with the guilty secret his tongue may never tell? He who has made you the repository of an honourable secret—nothing is what he thinks he owes you, and nothing will he ever pay-you.'

H. N.

**P. Cornelius Tacitus**, erklärt von KARL NIPPERDEY. Zweiter Band. Fünfte verbesserte Auflage, besorgt von Georg Andresen. Berlin. 1892. Mk. 2.70.

A NEW issue of this excellent edition will be welcome to all students of Tacitus. The general merits of the work are already so well known and appreciated that it is only needful to say a few words on the special characteristics of the fifth revision of it.

The editor states in the Preface that the text differs from that formerly adopted in eighty-nine places, of which nearly half are either restorations of the Medicean text where it had previously been departed from, or are due to recent collation of the manuscript. In forty-seven remaining places a conjectural reading has been substituted either for the Medicean text or for some previously adopted conjecture. Thus Madvig is now followed in reading 'militare horreum' for 'militarium' in 14, 33; in inserting 'pacis' rather than 'cladis' in 15, 13; and in reading 'aut redierat plebi spes' in the corrupt passage in 15, 40. Dr. Andresen also now follows Halm in adopting Prammer's insertion of 'melius' in 14, 20. Several new emendations originate from the editor himself, among which may be noticed the insertion of 'populi' before 'Romani' in 12, 60, and that of 'Domitiae, Neronis' before 'amitae' in 13, 27; the reading 'strepitu' for the corrupt 'repetitum' in 14, 61, the insertion of 'consentitur' after 'consequentibus' in 15, 54. The additions to the commentary amount to about ten pages in the whole volume, and are suggested chiefly by what has been brought forward recently by Mommsen and other eminent authorities. A few errors may be noticed which have still survived this latest revision: (1) in p. 24, note on 11, 18, 14, for 'Flavus,' 'Italicus' should apparently be read; (2) in p. 70, on 12, 29, 1, for ii. 89, read ii. 81; (3) in p. 71, note on 12, 31, 7, the Worcestershire rather than the Gloucestershire Avon should be mentioned; (4) in p. 78, note on 12, 40, 9, for H. i. 61, read H. i. 64; (5) in p. 145, note on 13, 39, 17, for H. iii. 71, read H. iv. 71; (6) in p. 189, note on 14, 24, 12, for Luc. iii. 273, read iii. 373; (7) in p. 224, note on 14, 65, 14, for H. iii. 20, read H. ii. 20.

H. FURNEAUX.

**P. Corneli Taciti Agricola**, edited with introduction, notes, and critical appendix by ROBY F. DAVIS, B.A., formerly scholar of St. John's College, Cambridge, assistant master at Weymouth College. Methuen & Co. London. 1892.

THIS edition is intended for the use of schools, and for boys beginning to read Tacitus; and the introduction and notes are on a scale suitable for such readers, and will probably be found to answer well to their requirements. The stages of the conquest of Britain before the time of Agricola might perhaps with advantage have been more fully given in the introduction, so as to throw light on the narrative in c. 13 foll., which is obscure from its brevity; and some connected view of the campaigns of Agricola

himself might well find place in it; space being found if needed, by some compression, in pp. xii., xiii. A few inaccuracies of expression may be noted, such as that Tacitus 'appears to have held a province...probably Germany' under Domitian; the government of these provinces being open to no one under consular rank. He might have been a 'legatus legionis' in one of the 'Germaniae' (though this is not very probable), or might have governed a lesser Caesarian province as legatus, or have held any senatorial pro-

vince, except Asia and Africa, for a year as proconsul. Also Mucianus should be called 'legatus' rather than 'proconsul' of Syria (p. 41), and Lentulus Gaetulicus should not be called 'the legatus of the Rhine legions' (p. 48), inasmuch as, as legatus of Upper Germany, he only commanded half of them. Such expressions however, it may be admitted, are hardly likely practically to mislead those for whom the book is intended.

H. FURNEAUX.

## NOTES.

IN the *Sitzungsberichte der Königlich Preussischen Akademie Der Wissenschaften zu Berlin*, November 3rd 1892, Professor A. Harnack republishes, with an introduction and commentary, fragments of two early Christian books the existence of which was previously known, the 'Gospel according to Peter,' and the 'Revelation of Peter.' These most important fragments were discovered in a grave at Akhmim, and first published by M. U. Bouriant, with two large fragments of the Book of Henoch, in the *Mémoires publiées par les membres de la Mission Archéologique Française au Caire*, vol. 9 fasc. 1, 1892. To Professor Harnack belongs the credit of having recognized them as what they are.

In the 'Gospel according to Peter' he signalizes four characteristics: (1) It claims to be written by Peter; (2) Its narrative nearly resembles, on the whole, that of the Canonical Gospels, but differs from them in its detailed account of the Resurrection; (3) It shows traces of Docetic influence; (4) It probably belongs to the second century A.D.

The fragment of the 'Revelation of Peter' contains the account of a vision, in which the condition of the departed is revealed to the Apostle. In some details Dante is anticipated in a remarkable way. This work too, Professor Harnack thinks, can

hardly have been written later than the middle of the second century. H. N.

[An edition of the above by J. ARMITAGE ROBINSON and M. R. JAMES is announced as about to appear immediately from the Cambridge University Press.]

\* \* \*

FIELD VOLES AND THE APOLLINE WORSHIP.—With reference to the interesting note of Mr. Warde Fowler it seems worth calling attention to *Pliny* x. 85, 1, 2. He remarks upon the wonderful way in which *mures* (he apparently knew of no distinction between mice and voles) breed, and continues: *Itaque desinit mirum esse, unde vis tanta messes populetur murum agrestium: in quibus illud quoque adhuc latet, quoniam modo illa multitudo repente occidat. Nam nec exanimis reperiuntur, neque exstat qui murem hieme in agro effoderit. Plurimi ita ad Troadem proveniunt; et iam inde fugaverunt incolae.* It would be very interesting to discover by the aid of the evidence of language at what date the common rat came into Italy—topo, the ordinary name for rat, is of course *talpa*; the Greeks call the animal *ποντικός*, showing that it came from the East; and the Slavonic nations have no common name for 'rat.' The Welsh call it the 'French mouse.'

HERBERT A. STRONG.

## ARCHAEOLOGY.

### EGYPT AND MYCENAEAN ANTIQUITIES.

THE controversy on this subject, which has been winding its way through many weeks and many journals, has not yet

reached a definite conclusion; but in view of its possible continuance over yet other six months, it may be useful here to summarize the points at issue, in the hope that readers of the *Classical Review* may see, without searching the columns of the *Academy*, the *Athenaeum* and the *Times*,

what these points are, and what is the present position of the disputants. The discussion arose out of Mr. Torr's criticism of Petrie's *Illahun* (*C.R.* March, p. 127), which was partly based on an article by Mr. Petrie in the *Hellenic Journal* xi. 270. Briefly stated, the main question is this: A certain class of pottery, of a distinctive technique, form and decoration, has been found, on the one hand at Mycenae and other 'Mycenaean' sites in Greece; and on the other hand at certain sites in Egypt. It is maintained by Mr. Petrie and others that the circumstances of the Egyptian discoveries enable us to assign a date of about 1400 B.C. to the 'Mycenaean' pottery of those sites and consequently to the Mycenaean civilization itself. Mr. Torr, as I understand him, is not occupied so much in denying the possibility of this early dating for Mycenae, as in arguing that the conclusion is not warranted by the evidence as yet adduced.

I propose to state first of all the facts upon which the supposed early dating for the 'Mycenaean' or (as Mr. Petrie calls it) 'Aegean' pottery is based; and then to give briefly the substance of Mr. Torr's criticism on each point. I should premise by saying that by the term 'Mycenaean' pottery is meant pottery of finely levigated clay with a polished surface on which decoration is laid by means of glazed paint; the shapes are easily recognized as distinct from those of other classes of pottery, as will be seen by reference to *Myken. Vasen*, Taf. xlv. The most characteristic, as well as the favourite, form is that which the Germans term *Bügelkanne*, and which we may call 'false-necked amphora.'

In discussing the date of Mycenae, the evidence naturally groups itself under three heads: first we have the literary evidence, that is, the light thrown on the monuments by Homer, and *vice versa*; secondly, the relations which the antiquities of Mycenae and correlated sites bear to those of the historic period in Greece; and thirdly, the relations which such antiquities bear to those of the other nations whose dates have independently been ascertained with comparative certainty. The first category lies outside the present enquiry, which is concerned with the monumental evidence; on this head, we need only refer to an article in the current number of the *Quarterly*, which is the most recent contribution to the Homeric aspect of the question. As to the second point, it is obvious that much must depend on the completeness of the chain of

evidence; and the further we go back, the more strain we must necessarily put on the few facts of which the chain is composed. Probably the oldest dateable Greek vase yet known is the Dipylon jug at Athens with an engraved inscription (*Ath. Mitth.* 6, Taf. iii.); this is assigned, for epigraphical reasons, to early in the seventh century B.C. It is generally agreed that the Dipylon style, at Athens and elsewhere, succeeded that of Mycenae, so that if we accept the earlier dating of Mycenae (1400 B.C.) we have a gap of at least 700 years; and though it is likely that the Mycenaean period was of long duration, it is difficult to spread such development as the authors of *Myken. Vasen* for instance trace, over so many centuries. Moreover, we have in one class of remains from Mycenaean sites,—the engraved gems,—a practically continuous development in art between Mycenae and historic times; and this would certainly not independently suggest such an interval. We are gradually learning more and more to distinguish the threads which connect Mycenae with historic Greece, in survival of forms, in systems of ornament, and most of all in the artistic instinct which is equally the birth-right of both. Are we then to imagine a pre-Dorian race in Greece, passing through eight centuries of declining art and wasting prosperity in struggles with a Dorian race who slowly but surely gained the mastery? That something of this nature occurred, we may readily believe; but the duration of this epoch is at least open to question.

The third category is that upon which the present controversy hinges. On this head it is contended in several quarters that the results of recent discoveries have settled the question finally in favour of Mr. Petrie's date; so much so that in the *Academy* of Oct. 29, Prof. Sayce actually uses this Mycenaean date as evidence for the dating of Hittite remains. 'Since' he says 'the discoveries of Dr. Petrie have now removed all doubt from the minds of competent archaeologists as to the early date of the Mycenaean antiquities...the parallels between the art of the Hittite monuments and that of the Mycenaean period are of considerable value in determining what we may call the Hittite age'—and yet in the same *Academy*, two pages further, is noted a paper by M. Heuzey in which attention is called to the resemblance between the subjects on the famous Mycenaean gold ring and a bas-relief in the Louvre with Hittite characters; M. Heuzey assigns the Hittite sculpture (and therefore presumably

the Mycenaean ring) to the ninth century: so that either Prof. Sayce is wrong, or M. Heuzey is not a 'competent archaeologist': which is by no means the general opinion.

The following is a summary of the points in question, with dates according to Brugsch's latest system of Egyptian chronology (*B.M. Guide* p. 50).<sup>1</sup>

(i) The Aah-hetep sword. A parallel is drawn between the inlaid daggers of Mycenae and an inlaid sword found near Thebes in 1859, with decorations and among surroundings which are apparently un-Egyptian in character and most nearly remind one of Mycenae. The mummy, which is stated by Birch (*Facsimiles &c.*) to have been found 15 to 18 ft. below the soil, contained the sword and other jewels deposited within the wrappings: it gives the name of Aah-hetep, and is the unique instance of this Queen's cartouche. On the other hand, the sword itself bears a name which looks like a misspelling of the first name of the founder of the XVIIIth dynasty (B.C. 1700); the same form, combined with this king's second name Ahmes, occurs on an axe found with the sword. When did she live? Some circumstances of the burial &c. would point to the XIth dynasty (before 2500 B.C.), but on the other hand, some objects in the mummy case have the name of Kames, others of Ahmes the first king of the XVIIIth dynasty (1700 B.C.). It is conjectured that Aah-hetep may have been the wife of Kames and mother of Ahmes.

(ii) At Ialysos in 1864 was found, among tombs containing Mycenaean objects, a scarab of Amenophis III. (1500 B.C.). At Mycenae have been found three separate objects in 'Egyptian porcelain,' all of which have indications of a royal name. The first is a scarab of Queen Ti, the wife of Amenophis III.; secondly, part of a vase with the end of a cartouche reading apparently [Amen]-hetep, with his usual title 'Lord of Thebes'; and thirdly, two fragments of a small slab which appear to contain parts of the name and titles of the same king.

(iii) In two graves at Gurob Mr. Petrie found instances of a false-necked amphora among deposits which showed the names of Amenophis III. (1500 B.C.) and Tut-ankhamen (B.C. 1466) respectively.

<sup>1</sup> I have followed the dates there given for the sake of convenience. It is however scarcely necessary to state that of the various systems of Egyptian chronology, not one can be accepted for dates much before the 8th century, except as a collection of rare and isolated facts strung together on a string of conjecture.

(iv) A 'Mycenaean' vase was found by Mr. Petrie in a tomb at Kahun (the 'tomb of Maket'), with various dateable objects, among which he proposes to find a limit of date between 1200 B.C. and 975 B.C. (*i.e.* Brugsch's 1333—966 B.C.).

(v) In his most recent excavations on the site of Tel-el-Amarna, Mr. Petrie has found a large quantity of fragments of Mycenaean pottery: since he finds no trace of anything later than Heru-em-heb (the last king of the XVIIIth dynasty), he concludes that it was deserted before the XIXth dynasty, and assigns 1400—1340 B.C. (*i.e.* Brugsch's 1500—1466 B.C.) as the extreme limits of date. All these data then would seem to favour the advocates of the earlier terminology; I may add two more, which in the English correspondence have not yet, I think, been quoted, viz.:

(vi) The wall paintings from three Theban tombs, quoted by Steindorff, *Arch. Anz.* 1892 p. 11, all probably of about the time of Thothmes III. (1600 B.C., given by S. as 'um 1470'): (a) Tomb of Reknara: with the 'nobles of the land Kefti and of the islands which lie in the sea,' bringing as gifts vases of gold and silver, different from the known Egyptian forms and related to the Mycenaean: they are dressed in the peculiar Mycenaean fashion. (b) An unpublished tomb: Prisse gives two vases from it as 'vases des tributaires de Kafa,' and these exactly correspond both in form and decoration with the Vaphio cups. (c) Tomb of an official who lived under Thothmes III.: here also are offerings brought by foreigners of rhytons and costly vases which, un-Egyptian in form and ornament, correspond best to Mycenaean.

(vii) In *Arch. Anz.* 1891 p. 37 Furtwängler says that in the Berlin Museum is a fragment of Mycenaean ware which comes from a 'stratum attributed with certainty by Egyptologists to the XVIIIth' dynasty, (1700—1400 B.C.). This fragment however was a donation from Mr. Petrie, and is therefore probably from one of the finds already before us and not fresh and independent evidence.

We now come to the points which have been advanced as direct evidence of the *later* dating.

(viii) On the wall paintings of the tomb of Rameses III. (about B.C. 1200) are represented five false-necked amphorae of blue glass and pottery, evidently Mycenaean in character.

(ix) In the British Museum (ivth Egyptian Room) is a false-necked amphora no.



22, 821 which, as the label states, was found at Dér el-Bahari in the tomb of one of the grandsons of King Pinetchem, who reigned about B.C. 1033. The tomb would consequently be about B.C. 970.

Proceeding now to review the evidence, we see that in the nine groups of facts we have, roughly speaking, fifteen instances of suggested dates, the outside limits of which are 1700—970 B.C. Of these fifteen, by far the largest proportion give a date between 1600 and 1400 B.C. If then we can accept as trustworthy the evidence upon which these dates are founded, there would be a strong presumption in favour of the early attribution of the Mycenaean civilization. It is certainly a remarkable coincidence that this Egyptian evidence, such as it is, should point so frequently to the three centuries after 1700 and leave absolutely unmarked the period between 970 and 700 B.C.

The evidence before us is threefold in character: royal scarabs or cartouches on objects found in tombs; Mycenaean pottery found in tombs or rubbish heaps; Egyptian wall paintings. The Aah-hetep sword (i) offers only a parallel of style to Mycenae, and, as such, would not rank as independent evidence; moreover, the circumstances of the burial in this case were of such a nature that it is doubted whether this can be regarded as an untouched contemporary record.

In the case of the tombs, it will be seen that while (ii) concerns Greek sites, (iii) to (ix) refer to tombs in Egypt. In the Greek tombs, we are dealing with imported objects; and it is obvious that such objects need not necessarily be of the period of the king whose name they bear; it is known that the names of famous bygone kings were often reproduced in antiquity as amulets; and the king so favoured at Mycenae and Ialysos happens to be one whose personality would have been very appropriate to a Greek usage of this kind; the name of Amenophis III. was more or less connected for all Hellenic posterity with the figure, so prominent in legend, of Memnon. On the other hand, it may be urged that this very prominence in Greek legend would seem to imply some original association of Amenophis III. with that portion of the Aegean or with people hailing therefrom; and that even if we may expect scarabs of 'Memnon' to have been eagerly bought in Greece many centuries after his death, we should scarcely expect to find those of Memnon's wife; and, as a matter of fact, it is stated by Erman<sup>1</sup>

that the name of this particular king has not otherwise been found reproduced upon works of later date. If it is urged that specimens of such long-posthumous scarabs have been found on Greek sites, we have at least one instance, in the Polledrara tomb, where a scarab has been generally accepted as contemporary evidence of date.

Objects found in a tomb may have come there in a variety of ways: they may have been heirlooms for generations, they may have been buried, dug up at any subsequent period, and then reburied in the tombs in which we find them; there is plenty of evidence of tombs having been thus ransacked in antiquity. Against this we must set the cumulative evidence of the four examples in (ii), in conjunction with the other evidence pointing to the date of Amenophis III. Of course it is always possible that a hoard of such cartouches may have been disinterred in antiquity, but this will hardly commend itself as a probability.

Lastly, there comes the question of style; are these cartouches accepted by Egyptologists as contemporary productions? On some of them, the hieroglyphics are so badly drawn or so ill arranged, as to suggest manufacture by workmen unfamiliar with the name they have written, or who regarded it as merely ornamental. Here we may revert to the sword of Aah-hetep (i), on which the hieroglyphics are also incorrect; in that case there are elements in the design which have an undoubtedly un-Egyptian appearance; and M. Daly (*Rev. de l'Arch.* 1860, p. 103) suggested that the sword must have been made by a stranger to Egypt. At Naucratis in the seventh century B.C. we have an instance of foreigners settled in Egypt reproducing imitations of Egyptian commodities; under the Theban Empire we know that foreigners were settled in Egypt; why should not these foreigners have had porcelain and scarab factories in Egypt, and have made there the objects for which, as the Greek finds suggest, there was a large demand in Greece?

Coming now to the question of the pottery, we have three definite examples (nos. iii, iv, and ix) of tomb-deposits in which 'Mycenaean' pottery is found in conjunction with objects which suggest a definite date; and no. v, in which a large quantity of such pottery is found on a site which apparently has not been occupied later than a given date; the full evidence as to Tel-el-Amarna has not yet been published, and we must therefore for the present suspend our judgment on this

<sup>1</sup> Quoted by Winter in *Arch. Anz.* 1891, p. 39.

point. Similar evidence has been offered as to the general mass of pottery found at Kahun. Arguments of this nature however (from pottery found on sites where the occupation was presumably of limited duration) must be received with caution; for instances are not unknown of Mycenaean fragments being found on a site and among objects which cannot apparently be earlier than the sixth century B.C.<sup>1</sup>; and yet none I think would seriously argue that Mycenaean pottery was in use in that century. As to the tomb-deposits, we may take it as a principle that the contents of an untouched tomb cannot be earlier than the latest object found with them; Mr. Torr consequently treats the tombs in question as proving merely that the pottery was buried after the date named; such burial may conceivably have taken place a long time subsequently, seeing that tombs were so used at various periods, and that one at least of these (the tomb of Maket) contains deposits of various dates.

Finally, we have the wall paintings; here again the evidence depends on a similarity of style, which may or may not be accepted: it has been suggested that the similarity would be still more striking if these paintings had not been the work of an Egyptian artist who was probably unfamiliar with the character of the objects which he was copying; at any rate we may take it, that at the time when these paintings were being executed, the objects depicted were being made by foreign nationalities, and were chosen by the wall painter as specially characteristic of the foreigners in question. If we accept this evidence, then we are confronted with the problem that the 'Mycenaean' civilization was in existence at any rate from 1600 to 1200 B.C. On the other hand, we have the direct evidence of no. ix, the tomb of the grandson of King Pinetchem, which would bring the lower limit of date down to about 970 B.C.: we thus obtain apparently a proof that the art which we call Mycenaean lasted over at least six centuries. If this were so, it requires no great stretch of the imagination to extend it over a further century or so either way. But with an art of so long duration, found over so wide an area, it is extremely hazardous to argue relative dates from classification based upon a regular course of development: the authors of *Myk.*

<sup>1</sup> See the account of the excavations on the site of the temple of Athene Cranaia (*Bull. de Corr. Hell.* 1888, p. 44). Among the fragments of pottery from Kahun (assigned by Mr. Petrie to the twelfth dynasty, 2500 B.C.) are some which compare best with Naukratis vases of sixth century B.C. (see Petrie in *J.H.S.* xi. pl. 14; and A. S. Murray, *Handbook* p. 30).

Vasen suggested such a classification, but even if this were proved, it would cease to be a general test if once we show that all the 'Mycenaean' pottery was not manufactured on the same site. Moreover, such an art might be expected to have left more traces of its existence in Greece; and it is certainly remarkable that we have not the scarabs of other Egyptian kings from Mycenaean sites: on the other hand, we may recollect that, while fresh evidence of Mycenaean remains is almost daily accruing there, very few Mycenaean sites have yet been thoroughly excavated; and the question, who the true representatives of the Mycenaean civilization really were, still remains to be settled. The present discussion is one of date, not of race; but we may note that the tendency of late has been to look towards Syria as having exercised at least some influence on the civilization which we call 'Mycenaean,' and which may have had its centre in Krete (Winter in *Arch. Anz.* 1891 p. 38).

CECIL SMITH.

MR. CECIL SMITH has kindly allowed me to see the foregoing article in proof, and permits me to append this note. His main difficulty appears to be that no Greek vase can be dated before 700, while no Mycenaean vase can be dated after 970; and that such an interval is inexplicable. The date 970 is proposed for the vase from the tomb of Pinetchem's grandson on the ground that Pinetchem was on the throne about 1033 by Brugsch's system of chronology. Brugsch holds that Dyn. 21 was succeeded by Dyn. 22 about 966, and that Dyn. 22 was succeeded by Dyn. 23 about 766. But if 22 was a *Nebendynastie*, as Lieblein suggests, there was not any interval between 21 and 23, and 200 years must be subtracted from Brugsch's date for Pinetchem. The interval would thus be reduced from 270 years to 70 years.

A tabular statement of Lieblein's results will be found at the end of his *Recherches sur la Chronologie Égyptienne*. He published his theory about the Twenty-Second Dynasty in the *Revue Archéologique* for 1868, vol. xviii. pp. 272 ff.

CECIL TORR.

*A Companion to the Iliad for English Readers.*  
By WALTER LEAF, Litt. D. London and New York: Macmillan and Co. Crown 8vo. Pp. xii., 411. 7s. 6d.

THE *Companion*, as Mr. Leaf remarks, is strictly a companion to another book, and

cannot be read as a separate work. In other words, it consists of the introductions and notes belonging to the prose translation of the *Iliad* by Messrs. Lang, Myers and Leaf. There is a long introduction to the poem itself, and short introductions to each book of it; and there are notes to all the principal passages, each with its cross-reference to the page and line in the translation. A work of that sort cannot be criticized as a whole. So we will direct our attention to what is certainly the most attractive part of it, namely, the author's account of the Homeric question.

Mr. Leaf recognizes three strata in the *Iliad*, besides various substrata. In the first place there is the 'Wrath of Achilles' occupying books 1, 11, 16 and 22. In the second place there are 'tales of the prowess of individual heroes' in books 2 to 7, 13 and 17. And in the third place there are 'great individual poems, led up to and connected by portions of narrative which are in themselves treated as subordinate.'

The following are the most notable of the 'fundamental discrepancies' on which Mr. Leaf relies in his analysis. In book 9 Achilles refuses the gifts of Agamemnon, yet in books 11 and 16 he speaks as though no gifts had been offered. If the offer was made, he afterwards behaved abominably. If not, his conduct was pardonable or even praiseworthy. The matter 'fundamentally affects our whole conception of the character of Achilles.' The man who wrote books 11 and 16 could not have written book 9 unless he was 'incapable of clearly realizing his own characters.' Again, in book 20 Achilles behaves contemptibly on the field of battle, whereas in book 1 he seems incapable of such irresolution and timidity. And the inference is that the man who wrote book 1 did not write book 20.

This brings us to the question whether Homer was one man, or three men, or perhaps a hundred. Mr. Leaf states his own opinion clearly enough:—'it is not inconceivable that one poet may, during the course of a long life, have composed the *Iliad* piecemeal: the probabilities seem to me strongly against it: the change in the attitude of the poet towards his own work seems more than we can credit to one man.' He thus repudiates the notion that a critic has only to point out inconsistencies in order to prove that more than one poet had a hand in the work. His test is that the inconsistencies must be so fundamental that we cannot imagine a poet capable of

such changes in his attitude towards work of his own making. But obviously this is not a test which can lead to any sure results, for nobody can tell how far a poet might change his attitude towards his work. At this rate, the whole thing is merely a matter of opinion, and can never be anything else.

But the question can be approached from another side. Mr. Leaf appeals to archaeology and history for proof that the first and second strata were formed in Greece Proper before the Dorian invasion, while the third stratum was formed afterwards in Asia Minor. And in that case the poem cannot be the work of one man.

According to Mr. Leaf, the Dorians invaded Greece about 1000 B.C., and thereby put an end to the dominion of the Achaeans: the Achaeans had attained the height of their civilization in the twelfth and thirteenth centuries, and had conquered Greece before 1500 B.C.: Greece had previously been held by the Pelasgians.

Mr. Leaf says that the Dorian invasion took place about 1000 B.C. 'according to the traditional chronology.' There is no 'traditional chronology' to which we can appeal in this way. In a well-known passage Clement of Alexandria remarks that the interval between the return of the Herakleidae and the expedition of Alexander was 715 years according to Phaneias, 735 years according to Ephoros, 820 years according to Timaios and Kleitarchos, and 770 years according to Eratosthenes.<sup>1</sup> These dates range from 1049 to 1154 B.C.; and other dates were assigned by other authorities, but none of them much nearer to 1000 B.C. Yet it is clear that Mr. Leaf is not using 1000 as a round number, for he relies upon this date in arguing that the Achaian strata in the poem were formed about 1050 B.C.

No doubt, there was a Dorian invasion of some sort at some date, for there were Dorians in Greece in historic times, and they can hardly be reckoned as aborigines. But we should like a little proof that the invasion was—to use the old phrase—a single and literal event, having its assignable date, and carrying at one blow the acquisition of Peloponnesos. And still more should we like a little proof that the Dorians found Peloponnesos under the dominion of the Achaeans. Ancient authors may be cited to this effect; but here they are not speaking of matters within their knowledge. The Greeks whom they knew were divided into

<sup>1</sup> *Strom.* i. 21, p. 145.

three clans, Aiolian, Dorian, Ionian; but in the *Iliad* they found them reckoned as a single clan, Achaian. Consequently, if they were going to treat the poem as a work of history, they had to place the events in the age before the inroad of the Dorians. And they would certainly have thought it impious to treat the poem as a work of fiction.

Mr. Leaf says 'it seems to be made out that about 1500 B.C. the Achaians were allies of the Libyans in a great invasion of Egypt—if the Achaians could invade Egypt, there is no antecedent improbability in their invading Troas.' He is here referring to the identification of the Achaians with the Aqaiasha. The Aqaiasha are mentioned in inscriptions relating to the invasion of Egypt in the fifth year of King Merenptah, and are not mentioned elsewhere. There is nothing in the inscriptions to show who the Aqaiasha were, or whence they came. The date 1500 B.C. was assigned to Merenptah by Lauth, and is far the highest of the dates assigned to that king under the various systems of chronology. A few lines before, Mr. Leaf has adopted the date which has been assigned by Mr. Petrie, from comparison with Egyptian remains, to the shaft-tombs at Mykenai. That date is 1150 B.C.; and, under the system of chronology adopted by Mr. Petrie, this falls in the reign of Seti II., who was a son of Merenptah. But, no doubt, those methods of reasoning which have served to identify the Aqaiasha with the Achaians, will serve also to identify Merenptah with Methuselah.

When Mr. Leaf asserts that the Achaians attained the height of their civilization in the twelfth and thirteenth centuries B.C., he seems to be adopting the date proposed by Mr. Petrie for the Mykenai civilization, and then attributing this civilization to the Achaians on the ground that it resembles the civilization depicted in the *Iliad*. But, in the first place, Mr. Petrie has obtained that date by methods of reasoning which may fairly be described as questionable. And, in the second place, there are some 'fundamental discrepancies' between the two civilizations. For example, the people of Mykenai buried their dead, whereas the *Iliad* says that the dead were burnt.

In dealing with the Catalogue of the Ships, Mr. Leaf says that 'it is particularly remarkable that the islands mentioned as sending contingents to Troy are almost exactly identical with those where remains of Mykenai civilization have been discovered.' And he adds that 'this is a remarkable support to the belief that the Catalogue is a

real gazetteer of Achaian Greece, and that the Achaians were the people to whom the Mykenai civilization belonged.' Clearly, the alleged coincidence would not go to prove that the Mykenai civilization belonged to the Achaians unless the Catalogue were already known to refer to Achaian Greece; or that the Catalogue referred to Achaian Greece unless the Mykenai civilization were already known to belong to the Achaians. But the coincidence is imaginary. Seventeen of the islands have yielded antiquities of the Mykenai type: but only seven of these are mentioned in the Catalogue, namely, Aigina, Salamis, Rhodes, Karpathos, Kalymnos, Kos, and Krete: and a dozen others are mentioned there which have not yielded antiquities of that type. Moreover, the Catalogue describes Rhodes, Kalymnos, Karpathos and Kos as islands of the Herakleidae, the national heroes of the Dorians; and, when history began, these islands certainly were Dorian, and so also were Krete and Aigina. If we had to draw an inference from the Catalogue, the inference would be that the Mykenai civilization belonged to the Dorians.

In order to substantiate his theory, Mr. Leaf is not only bound to show that the Achaians were established in Greece before the inroad of the Dorians: he is also bound to show that the first authors of the *Iliad* lived there among them—not, as tradition says, among the Ionians of Asia Minor. And he argues thus:—

'But the Ionian emigrants were above all things expansive and commercial: their centres were Miletos, Ephesos, Kolophon, and the other great towns of the Asian coast: their ships and their colonies went freely over all lands, from the recesses of the Black Sea on the one hand to Marseilles and Spain on the other. To suppose that people thus overflowing with living energy should care or be able to remove themselves entirely from their surroundings and throw themselves into a description of the past without allowing a single allusion, or, so far as we can detect, a single anachronism to escape them, is to credit them first with a power of historic imagination, and next with means of archaeological research, such as have been hardly equalled in the history of the world.'

This is a description of the Ionians as they were in the seventh century B.C. and afterwards. Their ships did not reach Spain before 630, Marseilles was not founded until after 600, and none of their colonies on the Black Sea appear to have been founded so early as 700. There is nothing to show that the energy which overflowed in this way in the seventh century, was already overflowing in the eighth or ninth. So, when Mr. Leaf proceeds to



doubt whether 'people thus overflowing with living energy' would have cared to sink the present in the past, the answer seems to be that they may not have been endowed with all this energy at the period in question; and that, even if they were, there is no saying what aspect of life might have taken their fancy in a poem. Mr. Leaf also doubts whether they would have been able to sink the present so completely in the past. But he does not suggest that they were inferior to the other Greeks of Asia Minor in 'historic imagination' or 'archaeological research'; and he credits Asiatic Greeks with the whole of his third stratum, which amounts to more than half the poem. If these Asiatic Greeks had enough of this imagination and research to produce one half of the poem, they had enough to produce the other half; and if the task was not beyond them, it was not beyond the Ionians. But all this talk about 'the past' seems to be founded on the notion that the Mykenian civilization was extinct before 1000 B.C., and that this is the civilization depicted in the *Iliad*.

The Greeks themselves always imagined that the *Iliad* was Ionian, not Achaian. Mr. Leaf admits that 'on this point the voice of antiquity was indeed unanimous.' And he is good enough to say that 'we cannot afford to neglect entirely testimony such as this.' Accordingly, he proceeds to 'explain how the mistake arose.' It was simply because the Greeks of classical times got the poem from the Asiatic Ionians, and found that it was mainly in their dialect. The Ionians had appropriated the work of the Aiolians, the Aiolians were the descendants of the Achaians, and the words which Fick calls Aiolic really belong to the old Achaian dialect. This is nothing but a series of guesses, without an attempt at proof.

While Mr. Leaf thus makes light of the opinion of the ancients that the *Iliad* was Ionian, he hardly condescends to notice their opinion that it was the work of one great poet. Yet these matters were not beyond the range of their information. Herodotos can probably be trusted when he says that Homer lived within four centuries of his own time, in other words, that the poet was an old man about 800 B.C. And we venture to think that the internal evidence agrees better with that date than with any other.

CECIL TORR.

HORACE, *Ep.* I. x. 49.

*fanum Vacunae.*

IN the archaeological summary in the *Classical Review* for May Mr. Walters makes a statement to which a recent visitor to the valley of the Licenza must be allowed to take exception. Speaking of the rediscovery of a votive inscription to Vacuna (*C.I.L.* ix. 4636) at Laculo (la Posta) in the Sabina, Mr. Walters adds, 'There seems however to be no reason for doubting that here we have the *fanum Vacunae* by which Horace wrote the tenth Epistle of the first book.' Were this the first discovery of a shrine of Vacuna, one might pardon the thought that Horace may have written the Epistle while on an excursion, in spite of the distance from his own valley. But why can there be any less doubt in identifying Horace's particular *fanum* with Poggio Fidone, in the valley of the Canera near Rieti, where two similar votive inscriptions have been found (*C.I.L.* ix. 4751-2)? The scholiasts' '*Vacuna apud Sabinos plurimum colitur*' is a sufficient warning against hasty identification, and makes it surprising that the Sabine collection of the *Corpus* contains but three Vacuna inscriptions.

In justice to Sig. Persichetti, from whom Mr. Walters had drawn his information (*Notizie degli Scavi*, Nov. 1891, pp. 342-3), it should be stated that he has nothing to say about Horace's *fanum*. He even conjectures that there was another temple of the same goddess near by at Bacugno.

In view of the general agreement that the Epistle was written from the Sabine Farm, it is quite superfluous to call attention to the distance between Licenza and Laculo (la Posta). As the crow flies the distance cannot be less than 35 m.p.—further, in other words, than from Licenza to Rome (25 m.p. in a straight line to the Porta S. Lorenzo). But the actual distance is, of course, much greater. Striking across from Licenza to the Via Salaria at its most accessible point—perhaps Osteria di Nerola—one would have first at least a dozen miles of hard travelling, and then 44 m.p. further along the Salaria to la Posta.

To the vexed questions of Horatian topography nothing can be added by an unsupported *non est dubium quin*.

F. G. MOORE.

Yale University.

THE PORTRAITS IN MR. WARDE FOWLER'S *JULIUS CAESAR*.

THE publication of Bernoulli's *Römische Iconographie* has introduced a scientific method into the study of Roman portraits in place of the old haphazard one of conjecture mainly based upon tradition. But, to judge from the illustrations to Mr. Warde Fowler's *Julius Caesar* in the 'Heroes of the Nations' series, the latest results of the new method have not found their way to America. Perhaps therefore I may be permitted to point out that in every instance, except one, in which the publishers of *Julius Caesar* have borrowed a portrait from Baumeister's *Denkmäler* (the first volume of which is eight years old), the evidence in favour of its representing the person to whom it is assigned is either doubtful or negative.

The famous statue in the Spada Palace (p. 48) bears no resemblance to the head of Pompeius on coins, or to the one certainly authentic bust of him in the Jacobsen collection at Copenhagen; see *Röm. Mittheil.* i. 37 (1886), Reinach, *Mithridate Eupator* p. 376, and in *Revue Archéologique* xv. 339 (1890). The fine bust at Madrid which appears opposite to p. 72 of Mr. Fowler's book was till quite recently accepted as a genuine bust of Cicero, but its attribution rested solely on the inscription. It has now been shown that this does not belong to the bust. The attribution therefore falls to the ground and with it that of the busts in the *Museo Chiaramonti* and at Apsley House which evidently represent the same person as the Madrid bust. So far as our knowledge goes at present, there is no authentic or even probable portrait of Cicero in existence.

The other two illustrations to which I would call attention are the portraits of M. Antonius (p. 256) and M. Brutus (p. 268). They both belong to the doubtful category. In the Uffizi bust of Antonius we miss the low forehead and the hook-nose which Plutarch ascribes to him and which we find faithfully reproduced in the coins and in a gem of the British Museum. The arrangement of the hair too is quite different, and the only points of resemblance are the thick neck and projecting chin, the latter however being a restoration. There is more to be said for the Capitol bust of Brutus, of whom also we have portraits on coins and on a gem in the British Museum. The hair combed over the forehead, the thin cheeks, 'the lean and hungry look' which according to

Plutarch characterized both him and Cassius, are there; but in other respects, especially as regards the shape of the nose, there is considerable difference from the coins. On the whole, apart from any external evidence, there is not sufficient resemblance to warrant us in claiming this bust as an authentic portrait of Brutus.

Of the portraits of Caesar Mr. Warde Fowler has himself spoken in his preface. I will only add that the bust of a *pontifex maximus* in the *Museo Chiaramonti* (p. 78) is evidently that of a much older man than Caesar lived to be.

I am strongly in favour of historical works being illustrated, but illustrations which are not genuine are worse than useless. The true busts of Pompeius reveal the emptiness of the man so unmistakably that they are of real importance for the understanding of his character. It is therefore a pity that the Spada statue, the head of which by the way does not belong to the body, should continue to be handed down as his likeness.

A. TILLEY.

*The Origin of Metallic Currency and Weight-Standards.* By WILLIAM RIDGEWAY. Cambridge, 1892. Pp. 417. 8vo. (with 60 illustrations).

THIS remarkable book commends itself to the reader by its originality and freshness of tone not less than by the evidence it affords of wide and varied research. The author's aim is to arrive at a knowledge of the origin of metallic currency and weight-standards by the Comparative Method—a method almost entirely neglected by numismatists and metrologists, who have, at most, compared the Greek systems with those of Babylonia and Egypt. The theories of Boeckh and of the later writers on metrology start with the assumption that the ancient systems were obtained scientifically. Professor Ridgeway, on the other hand, contends that they were arrived at empirically and maintains that the Greeks, Babylonians and other nations originally adopted for weighing and measuring those primitive processes that the anthropologist finds everywhere in vogue among savage and semi-civilized peoples. 'The Babylonian measures of capacity (say the upholders of the current theories) and their system of weights were based upon one and the same unit as their measures of Time and Space, and as they are believed to have determined the

length of an hour of equinoctial time by means of the dropping of water, so too it is conceivable that they may have fixed the weight of their Talent, their Mina and their Shekel as well as the size of their measures of capacity, by weighing or measuring the amounts of water which had passed from one vessel to another during a given space of time.' But this, as Mr. Ridgeway points out, is not only mere theory but theory at variance with all that we know about the actual methods employed by men in their early attempts at weighing and measuring.

Mr. Ridgeway takes Homer as a kind of text for his elaborate inquiry. In the Homeric Poems mention is made of two units of value—the cow (or ox) and the Talent. The cow, as the analogy of barbarian communities shows, was clearly the primitive barter-unit employed before the precious metals were used as media of exchange. The Talent (in Homer) is only mentioned in relation to gold, and—there being no question of coined money—all values are expressed in oxen. Yet though the value of commodities is never expressed in talents, it can hardly be doubted that talents of gold circulated freely. A solution of this difficulty is found by Mr. Ridgeway in the explanation that the talent of gold represented, and was of the same value as, the older ox-unit. Even after the actual bartering of cattle had ceased and had been superseded by gold as the medium of exchange, values continued to be expressed in oxen. In the same manner, the Caucasian Ossetes, though long accustomed to money, kept their accounts in cows—5 roubles being reckoned to the cow. The Homeric gold Talent, of about 130 grains, thus equated to the value of the primitive unit the ox, is, according to Mr. Ridgeway's highly probable view, the origin of the later Euboic system, the standard, namely, that was always employed by the Greeks for weighing gold, but which has hitherto been supposed by metrologists to be the light Babylonian shekel. So also the Aeginetan standard is shown to be indigenous and to be likewise based on the Homeric *talanton*.

Mr. Ridgeway is then led on to inquire into the nature of primitive currencies, and finds abundant evidence that in Asia, Europe and Africa the cow was the chief unit of barter. The regions of the ox or cow unit are further shown to be those over which gold was equally distributed, and it is next demonstrated that the art of weighing was first employed for gold, and that

the units of the standards by which, throughout these regions, gold is weighed are practically of the same weight as the Homeric talent, *i.e.* 130—135 grains of gold: in a word, that the gold-unit is everywhere based on, and is the equivalent of, the ox or cow.

In the latter part of his book Mr. Ridgeway criticizes with much effect the theories of metrologists as to the origin of the Egyptian, Babylonian, Greek, Italian and other systems—theories which all pre-suppose for those systems a scientific basis. Of several new explanations offered by Mr. Ridgeway perhaps the most ingenious and convincing is the view that the Roman *As* was originally a rod or bar of copper of definite measurements.

In his chapter on the Greek system Mr. Ridgeway makes a bold but, as it seems to me, ineffectual attack upon the orthodox view as to the meaning of Greek coin-types. The view now commonly held and ably set forth in the writings of Burgon, Curtius, Gardner and Head is, of course, that the principal devices on Greek coins are generally to be explained by reference to the cultus and myths of divinities and heroes. Mr. Ridgeway admits that this may be a true explanation of a certain number of coin-types, but denies that it is of universal validity. In part, he seems to revert to the theory of the old numismatists who saw in many, if not in all, types a direct allusion to the commerce and natural productions of the coin-issuing town or state. According to this view, when a bunch of grapes and a wine-cup appear as the coin-types (*e.g.*) of the Aegean Islands, they do not signify that those islands worshipped Dionysos but that they cultivated the vine with success and engaged in the wine trade. But Mr. Ridgeway goes beyond this and suggests the ingenious explanation that many early Greek coin-types are not only of commercial import but reproduce actual barter-units whose use had been superseded by the introduction of coinage. To quote his own words:—'The fact of the occurrence of the type of the cow or cow's head on early Greek coins is evidence that the early monetary unit was the ox.' 'The cow or bull's head on the early gold and electrum [coins] was the indication of the value. In later times when the connection between ox and coin was only traditional, the ox was put on coins simply as symbolical of money.'

The frequent occurrence of the ox-type on coins cannot be denied, and at first sight it certainly seems as if the early coin-engraver had a keen sense that the original *pecunia* was

*pecus*. But are we justified in adopting this tempting theory when we remember how common animal-types are not only on early Greek coins but on other monuments of archaic Greek art? Nor, in fact, is the ox the only animal-subject affected by the early coin-engraver. I should say, for instance, that the lion was quite as often portrayed on archaic coins as the ox or cow; and even if we go no further than the well-known Santorin hoard of seventh and sixth century money, we find not only a lion, but a goat, a dolphin, a boar, a horse, an eagle, a cock, and a tunny. On page 332, fig. 46, Mr. Ridgeway engraves the obverse of a coin of Lycia with a boar and suggests that this represents a Lycian barter-unit. But here, again, it is unsafe to insist on the significance of this particular animal when we remember that Lycian coins are remarkable for the variety and frequency of their animal and monstrous types. This very Lycian coin has, in fact, on its reverse a triskelis of cocks' heads—a type which may or may not be solar, but which at any rate can hardly be explained in any commercial sense.

Want of space precludes an examination of all the types that Mr. Ridgeway explains as barter-units, but I select three or four that he will, I trust, consider to be fairly crucial tests of his theory.

(i). *Tenedos*. The island of Tenedos, says Mr. Ridgeway, 'struck at a very early date silver coins bearing for device a double-headed axe,' and he suggests that this type is the representative of a primitive axe-currency that existed in the island. But in regard to this statement it is important to notice that the double-axe is not the only device on Tenedian coins. There is another type—a janiform head—which occurs as early and as frequently as the double-axe, and which in fact forms the obverse type of those coins of which the double-axe is the reverse. The janiform head is usually explained as Dionysiac, and, whether this is a correct explanation or not, it is clear that the type can have no connection with the commerce or primitive barter usages of Tenedos. This being the case, it seems much simpler to explain the double-axe as having some significance in myth or cultus. Mr. Ridgeway indeed very fairly admits in a note that 'the axe was often used as a religious emblem.' And further, the double-axe ought, according to Mr. Ridgeway's theory, to appear on the earliest coins of Tenedos, that is, on those issued when the remembrance of the axe-currency still remained fresh. But, unfortunately for the

theory, the earliest coins of Tenedos—those with the incuse square—do not display the double-axe but simply the janiform head.

(ii). *Chios*. At Chios, writes Mr. Ridgeway, 'a wine-jar is a regular adjunct of the mintage'; Chios was famous for its wine and it may be conjectured that a wine-cup or measure was the old unit of barter employed in the island—the earliest coin-types thus indicating 'the object (or its value) which the coin replaced.' But in reference to this, I would urge that it is not legitimate to explain the wine-jar adjunct without also explaining the Sphinx—a type by which, on the earlier coins, the wine-jar is invariably accompanied. And further we ought, in order to satisfy Mr. Ridgeway's theory, to find the wine-measure or jar on the earliest coins of Chios. But on the earliest electrum and silver money of the island the wine-jar is altogether absent and only the Sphinx appears.

(iii). *Thasos*. Thasos, like Chios, 'was famous for its wine and accordingly the wine-cup is a regular adjunct of its coins.' This statement again is wanting in numismatic precision. A wine-cup is, indeed, the type of the later coins of Thasos, but its earliest coins—those struck B.C. 550-465—have a representation of Silenos carrying off a nymph—a type suitable to a people devoted to the orgiastic rites of the Thracian Bacchus, but one which would form a somewhat roundabout advertisement of the merits of the Thasian vintages.

(iv). *Cyzicus*. Lastly, the tunny that is impressed on the coins of Cyzicus is regarded by Mr. Ridgeway as an indication that these coins superseded a primitive system in which the tunny formed a monetary unit, just as did the stock-fish in mediaeval Iceland. The existence of such a unit in a city like Cyzicus that derived its revenues largely from the tunny-fisheries is *a priori* likely enough, and in this case it must be admitted in Mr. Ridgeway's favour that the tunny occurs not merely on Cyzicene coins of a comparatively late period but on the earliest pieces of its money. But is an interpretation from the sources of myth or ritual entirely precluded? Mr. Ridgeway answers that it is: yet his own engraving (fig. 32) of the earliest electrum coin of Cyzicus seems to negative his view. The tunny on this coin is tied with fillets, and I am sure that Mr. Ridgeway (though he hints at *jugged* hare on the coins of Messina) will not say that this tunny is filleted merely in a base culinary sense. On coins, as on other Greek monuments, the fillet is a plain indication



that the object to which it is attached is sacred to a divinity. Thus (to deal only with coins) the fillet is found attached to the temple statue, to the sacred temple-key, and to the bull destined as a sacrificial victim. Why then may not this tunny be an offering to some divinity of Cyzicus—an offering of the first-fruits of the rich harvest of the sea? We have not, it is true, any literary record of such an offering at Cyzicus, but at any rate some such practice was not unknown in Greece, as may be seen from a passage in Athenaeus, quoted by Mr. Ridgeway, and from other mentions in the authors referred to in Rhode's monograph *Thynnorum captura* (Leipzig, 1890), p. 31 and p. 51.

For these and for other reasons that could be given I am unable to accept Mr. Ridgeway's theory as to the meaning of Greek coin-types. I have dwelt on it at length because it is very interesting to numismatists and one moreover that is likely to appear plausible to those who are not students of numismatics. At the same time, I should make it clear that this theory is by no means the most important feature in Prof. Ridgeway's work—a work which undoubtedly makes an epoch in the study of early weight-systems and currencies.

WARWICK WROTH.

*Excavations of the American School of Athens at the Heraion of Argos, 1892.* By CHARLES WALDSTEIN, Litt.D. etc. New York and Boston: Ginn and Co. London and Berlin: Asher and Co. 1892.

BRIEF notices of the American excavations at the Heraion at Argos have already twice appeared in the *Classical Review* (1892, pp. 280 and 424). It is now widely known that the main interest of the discoveries falls under two distinct heads. We have first antiquities found on the site of the second temple built by Eupolemos, including fragments of the temple architecture and sculpture; next the charred remains of the first temple (burnt in 429 B.C.); and, connected with these by date, though found on the slope of the west end of the second temple (at a depth of between 10 and 15 feet below the surface), a mass of objects of distinctly archaic and in part prehistoric character. To none of these last is Dr. Waldstein inclined to assign a date as late as the 5th century B.C. The black layer of earth in which these miscellaneous objects, e.g. terracottas, bronzes, ivories, gold rosettes, por-

celain images, coins, etc., were found, may very likely have been the *débris* from the earlier temple, shot down over the supporting wall.

In the preliminary publication before us—published with a promptitude for which all archaeologists will be grateful—Dr. Waldstein gives a selection from both periods. He devotes however most space and attention to the more obvious interest of the fifth century find. Dr. Waldstein indeed holds a brief for this fifth century. In a sentence of consummate obscurity (p. 2) he warns us against the danger, to mythology as to art, of beginning at the beginning. We will follow his lead and take first the second temple, reserving for a second number the precarious joy of discussing the problems of prehistoric mythology which the Heraion excavations naturally suggest.

The site chosen was a happy one. No time had to be lost in determining the general lie of the temple. In 1854, when M. Rhangabé made tentative excavations, portions of both the supporting and peribolos walls were visible, and were still visible in the spring of the present year. It may be said generally that when money is—as it always is for excavations—scarce, it is far better to begin with these obvious sites where remains lie above ground. We may also add in passing that Dr. Waldstein records his conviction that to work even a short time with large gangs of men is the more economical and generally more satisfactory method.

Over four out of the twenty pages of text are devoted to the detailed analysis of the head of 'Hera.' Over this head it is pretty sure that controversy will rage. Dr. Waldstein holds that 'in all its characteristics this head manifests that it is neither archaic, nor transitional, nor of the fourth century B.C., nor archaistic or belonging to the later renaissance of earlier Greek types: but it is clearly the work of an artist living in the fifth century.' Further that 'until it can satisfactorily be proved not to be so, we have reason to consider it a representative of the Argive school of art of the second half of the fifth century, and as such to hold some relation to the work of Polykleitos, who established the ideal type of Hera in this temple.' Further Dr. Waldstein considers it 'possible' that the 'Hera' figure stood on the pediment at the west end immediately below which it was found. The questions here raised will be difficult to determine, depending, as they do, so much upon art criticism. The present

writer after long looking at the cast of the head in the Fitzwilliam Museum declares for Dr. Waldstein. His view is of course—and he knows it himself—based on three uncertainties: *i.e.* the uncertainty if the head be a Hera or not, if it belong to the pediments or not, and the uncertainty of the relation between Polycleitos and those pedimental sculptures. But after all Dr. Waldstein relies on the logic, not of historical evidence, but of visual impression.

One word as to the head representing Hera. If the Hera Farnese and the Hera Ludovisi represent the goddess, the difficulty raised by the discoverer, that the head is rather maiden than bride, seems to us to be no difficulty at all. As will be seen in the second paper, Hera was much besides matron; and we need only recall that at Stymphalos there was an ancient cult, reputed Pelasgian (Pausanias viii. 22, 2), of Hera in her triple aspect as maiden child (*παρθένω μὲν ἐν οὔρῃ παιδί*) as well as wife and even widow, to enable us to recognize in her the year-goddess in the three Greek seasons, spring, summer-autumn, and winter. At Nauplia too (Pausanias ii. 38, 2) year after year she renewed, as every year-deity must, her youth and maiden-hood by bathing in the spring Kanathos. The exclusive matron-hood familiar to us in the *Iliad* is but one aspect, emphasized to complete the literary Olympian family circle.

After this head, the next fragment of importance is the metope on plate vi. From a phototype only it would be precarious to criticize details of style. We are not told if sufficient fragments have been found to allow of an attempt at reconstructing any other metopes or the pedimental sculptures. It is very fortunate that Pausanias (ii. 17, 3) states the subjects that were depicted 'above the pillars.' This expression—*ὑπὸ σταῖς*—Dr. Waldstein takes, and probably rightly, to mean both pediments and metopes. We think this, because one of the subjects named is the birth of Zeus—a subject of very special appropriateness, as we hope to show later, to a temple of Hera, but scarcely within the scope of metope composition. Dr. Waldstein thinks that the *τὰ μὲν* and the *τὰ δὲ* of Pausanias probably refer to the front and back pediments, and [metopes. This would give the birth of Zeus to one

pediment, the Gigantomachia to the corresponding metopes. To the other pediment would be given the subject *ἐς τὸν πρὸς Τροίαν πόλεμον*, which Dr. Waldstein somewhat arbitrarily assumes to have represented the Departure for Troy; leaving for the corresponding metopes the Taking of the city. For ourselves, *qua* subjects, we should prefer to take the Gigantomachia (cf. the pediment of the Megara Treasure House) for the second pediment and distribute the Trojan scenes round the metopes. Mixed with these were probably Amazon contests—a subject (as again we shall show later) of admirable fitness for a Hera temple.

Among the architectural fragments, the eye is immediately caught by the lovely little portion of a sima with the two birds perched on the volutes. Of course comparison is at once suggested with the charming fragment from the Erechtheion which used to lie to the right hand of the entrance gate to the Acropolis. Here only one bird remains and has an oddly accidental look; the Heraion fragment shows that a second bird must have stood with its back to the one remaining, heraldic fashion. Dr. Waldstein finds in the Erechtheion architecture the closest analogy to that of the Heraion: a photograph of the Erechtheion bird lies before us; the ornament seems to us more compact, and formal, and certainly a degree earlier in style. The presence of the bird on the Erechtheion, specially if it be earlier, forbids us to see in it the cuckoo Dr. Waldstein suggests. It is nothing but a fascinating bit of decoration. We believe it is proposed to publish shortly the fragments found by M. Rhangabé; to these must belong we suppose a similar bird now in the local Museum at Argos.

As to the plates, we may perhaps be allowed to enter one protest. If expense be any object, why devote three out of eight to views of the site? Pictures of masses of workmen with spades and barrows are of no manner of interest to any one. They do well enough in a magic lantern, but seem to us utterly out of place in a scientific publication. The vase fragments, requiring presumably colour, are postponed till next time, but are there not the terracotta plaques which we all eagerly look for?

JANE E. HARRISON.

(To be continued.)

SEVERAL important additions have been recently made to the Greek sculptures in the British Museum of fragments which have hitherto remained unidentified. These include several portions of the noble group of a Persian rider, which Sir Charles Newton found among the ruins of the Mausoleum: three considerable fragments have been readjusted in their place upon the near hindquarter, which add appreciably to the general effect. These additions are due to the Museum mason, Mr. Pinker. The latest readjustment is of peculiar interest, inasmuch as it concerns the Western Pediment of the Parthenon. Among the Elgin marbles there is a torso<sup>1</sup> from the Parthenon which has always been regarded as belonging to Metope No. XIV. Carrey's drawing of this Metope showed the figure of a Greek in a position somewhat similar to that of the torso, and Michaelis pl. 3, xiv. gives Carrey's drawing with the torso in its place. There the matter was considered as settled;

<sup>1</sup> *B. M. Cat.* No. 342, 2. Vignette to *Mus. Marbles* Pt. vii.

but a new attribution, which is undoubtedly the correct one, has now been proposed for the torso by an Austrian sculptor, Herr Schwerzek, who is spending a short time in London and is preparing to model a restoration of the Western Pediment. Herr Schwerzek noticed first that the torso is worked entirely in the round, and therefore cannot belong to the Metopes, and secondly that the forms are those, not of a grown man, but of a boy; from this it was but a short step to the figures of boys in the Western Pediment. We know from Carrey's drawing that there was originally a boy on each side of the seated figure (Lencothoe? *B. M. Cat.* 304 Q); at present only the lower limbs of this figure, with traces where the boy on her right leant against her, are remaining. But Carrey's drawing shows plainly that the torso of this boy (304 P) exactly corresponds with the sculpture in question. Most fortunately, sufficient surface of the original break still exists to turn the probability into a certainty, and the torso is now to be seen adjusted to its true position.

CECIL SMITH.

## LIST OF NEW BOOKS.

### FOREIGN BOOKS.

*Aristoteles.* Morale à Nicomaque, livre 10. Texte grec publié avec une introduction, un argument, des notes en français et un appendice par Hannequin. 16mo. 196 pp. Paris, Hachette. 1 fr. 50.

*Attinger* (G.) Essai sur Lycorgue et ses institutions. Gel. Neuchâtel 92. 52 S. 8vo.

*Beck* (J. W.) *Studia Gelliana et Pliniana.* 8vo. 55 pp. Leipzig, Teubner. 1 Mk. 60.

[Extract: *Jahrbücher für classische Philologie.*]

*Bulic* (F.) *Inscriptiones quae in C. R. Museo Archaeologico Salonitano Spalati asservantur.* Program. Spalato 92. 131 S. 8vo.

*Cicero.* Rede für T. Annius Milo. Für den Schulgebrauch erklärt von F. Richter und A. Eberhard. 4te Auflage bearbeitet von H. Nohl. 8vo. 110 pp. Leipzig, Teubner. 1 Mk.

*Corpus inscriptionum graecarum Graeciae septentrionalis, consilio et auctoritate academiae litterarum regiae borussicae editum.* Vol. I. Folio. Berlin, G. Reimer. 85 Mk.

[Contents: *Inscriptiones graecae Megaridis, Oropiae, Boeotiae* edidit G. Dittenberger. vii, 806 pp.]

*Czyckiewicz* (A.) *Untersuchungen zur 2te Hälfte der Odyssee.* Buch xiii. und xvii-xxiv. Program. 8vo. 54 pp. Brody, F. West. 1 Mk.

*Egli* (J.) Die Hyperbel in den Komödien des Plautus und in Ciceros Briefen an Atticus. Ein Beitrag zur Charakteristik der römischen Umgangssprache. Program. Zug 92. 38 S. 8vo.

*Ehrlich* (F.) Mittelitalien. Land und Leute, in der Aeneide Vergils. Program. Eichstätt 92. 82 S. 8vo.

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*Euripides.* Iphigénie à Aulis. Texte grec accompagné d'une notice, d'un argument analytique, de notes en français. 16mo. 123 pp. Paris, Hachette et Cie. 1 fr.

*Fahlberg* (A.) De Hercule tragico Graecorum. Dissertatio. Leipzig 92. 51 S. 8vo.

*Fischer* (C. T.) De Hannonis Carthaginiensis Periplo. I. Dissertatio. Leipzig 92. 54 S. 8vo.

*Förstmann* (A. W.) De vocabulis quae videntur esse apud Herodotum preticis. Dissertatio. 92. 72 S. 8vo. 1 Mk. 50.

*Froehde* (O.) Valerii Probi de nomine libellum Plinii Secundi doctrinam continere demonstratur. 8vo. 157—203 pp. Leipzig, Teubner. 1 Mk. 20.

*Gaebler* (H.) Erythra. Untersuchungen über die Geschichte und die Verfassung der Stadt im Zeitalter des Hellenismus. 8vo. v, 127 pp. Berlin, Mayer und Müller. 3 Mk.

*Gehlhardt* (P.) De adverbis ad notionem augendum a Plauto usurpatis. Dissertatio. Halle 92. 48 S. 8vo.

*Gomperz* (T.) Die jüngst entdeckten Ueberreste einer den platonischen Phaeton enthaltende Papyrusrolle. 8vo. Wien, Tempsky. 50 pfg.

[Extract: Sitzungsberichte der kaiserlichen Akademie der Wissenschaften.]

*Graf* (R.) Unregelmässige griechische Verba in alphabetischer Reihenfolge zusammengestellt. 8vo. iv, 21 pp. Stuttgart, Metzler. 70 pfg.

*Guilelmus Blesensis* Alda, comoedia edidit C. Lohmeyer. 12mo. 87 pp. Leipzig, Teubner. 80 pfg.

[Extract: *Jahrbücher für classische Philologie.*]

*Huggenmüller* (H.) Ueber den Fünfkampf der Hellenen. Program. München 92. 62 S. u. 1 Tafel. 8vo.

*Hergel* (M.) Die Irrfahrten des Menelaos mit Bemerkungen über die Komposition der Telemachie. Program. München 92. 41 S. 8vo.

*Herzog* (E.) Zur Litteratur über den Staat der Athener. I. Tendenz und Zusammenhang der pseudoxenophontischen Schrift über den Staat der

- Athener von K. 2, 19—3, 13 aus betrachtet. II. Ueber Aristoteles' Ἀθηναίων Πολιτεία. Programm. Tübingen 92. 83 S. 4to.
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